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References to literature should be listed alphabetically by authors at the end of the article; all references should be complete. For articles in journals or other serial publications, the reference should give in order: Author's name, year of publication, full title: name of periodical or series, volume number, inclusive pages. For books or other unnumbered publications, it should give: Author's name, year of publication, full title: place of publication, publisher. This form follows the recommendations in the pamphlet "Abbreviations used in publications of the U. S. Geological Survey", which is also the standard used for abbreviating names of periodicals, etc. Citations within an article should be to author and year, with specific pages wherever appropriate.

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REFERENCE

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In Memoriam

RICHARD SWANN LULL

1867-1957

Richard Swann Lull, Editor of the AMERICAN JOURNAL OF SCIENCE from 1933 to 1949, died at his home in New Haven on April 22, 1957 in the ninetieth year of his age.

He was born in Annapolis, Maryland on November 6, 1867, the son of Captain Edward Phelps Lull of the U. S. Navy. Educated at Massachusetts Institute of Technology, New Jersey State Model School, Rutgers College, and Columbia University, he received the degrees of B.Sc. and M.Sc. from Rutgers and of Ph.D. from Columbia. He later was awarded honorary degrees by both Yale and Rutgers.

He joined the faculty of Massachusetts Agricultural College at Amherst in 1894 as Assistant Professor of Zoology where he taught until 1906 when he came to Yale as Assistant Professor and Assistant Curator of Vertebrate Paleontology in Peabody Museum. Here he became Professor of Vertebrate Paleontology in 1911 and was Sterling Professor of Paleontology from 1922 until his retirement in 1936, serving also as Director of Peabody Museum of Natural History at Yale from 1922 to 1938.

Tall and powerful of build, he was a prominent athlete during his college days, both in football and track, and at one time he held the distance record for the 16-pound hammer throw. His fine physique and proud bearing made him a distinguished figure to the end of his active career. Gifted with a warm personality, a gracious manner, and a flair for expressing ideas in picturesque phrase, he became one of Yale's most gifted teachers and his famous course, Organic Evolution, was taken by more than 3,000 men, all of whom will remember him with affectionate esteem.

His bibliography of 205 titles includes several books, and many scientific papers in the field of vertebrate paleontology, notable among which are *The Ceratopsia* (with J. B. Hatcher and O. C. Marsh), *Triassic Life of the Connec-*

ticut Valley, A Revision of the Ceratopsia or Horned Dinosaurs, Hadrosaurian Dinosaurs of North America and *A Remarkable Ground Sloth.* His well known textbook, *Organic Evolution*, was widely used and his famous lecture, *The Pulse of Life*, will be remembered long after his passing. In 1933 he received the Daniel Giraud Elliot Medal of the National Academy of Sciences.

He was a Fellow of the Geological Society of America (Vice President in 1925), a Fellow of the Paleontological Society (President in 1925), a Fellow of the American Academy of Arts and Sciences and a member of numerous other professional societies.

The descendant of a long line of naval officers, he would have chosen a career in the navy except for the misfortune of an early illness that impaired his hearing. To the end of his days, however, the lore of ships remained his favorite hobby, and during World War I he found an opportunity to serve his country as Instructor in Seamanship in the Yale Naval Training Unit.

Professor Lull became the Editor of the AMERICAN JOURNAL OF SCIENCE in 1933 after the untimely death of Ernest Howe, and he served the JOURNAL with devotion until it became clear that the strain of reading proof was deleterious to his eyesight. In particular, during the difficult years of World War II when few scientists were writing papers for ordinary publication, he saw to it that the JOURNAL appeared regularly with articles of general interest. His colleagues in the Yale Department of Geology, the Peabody Museum, and the AMERICAN JOURNAL OF SCIENCE will remember him as a true scientist and a gallant gentleman.

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IN BEHALF OF THE RECENT*

R. B. MORRISON, JAMES GILLULY, G. M. RICHMOND, and C. B. HUNT

ABSTRACT. The proposal to reduce the rank of Recent from its present epoch-series status is reviewed and rejected on the ground that the present status is historically and pragmatically sound and that adoption of the proposal would lead to confusion in world literature and to obscurity of the stratigraphic record.

INTRODUCTION

The Quaternary has generally been regarded both in Europe and America as composed of two epochs: the Pleistocene and Recent. Some American Pleistocene geologists, however, advocate that Recent be abandoned as a formal stratigraphic term and that the time now so designated should be included in the Pleistocene or even in one or more sub-ages of the Wisconsin age. Flint (1948, p. 205-209) has summarized the argument for this viewpoint.

The authors of this note believe, however, that the arguments for retaining Recent in its present status as an epoch-series outweigh those for a reduction of its rank. In essence our argument is based on practical grounds—the necessity of having categories of relative rank to which local strata (and inferred events) may be referred. Enough ranks are needed for subdivisions of the Recent to justify its present rank and the proposed lowering would make such reference awkward and even nearly impossible. This operational convenience—the only really valid basis for any classification in natural history and the historical one in stratigraphy—thus fortifies, in our opinion, the already potent argument against the modification of any system of classification in world wide use unless it is shown to be actually illogical. This is surely far from the present case.

Recent was first proposed by Lyell (1833, p. 52-53) for the latest "era" of geologic time, although he originally defined it in the sense of both the Pleistocene and Recent epochs of the present classification. Later, however, he (1873, p. 3-4) differentiated the Recent from the Pleistocene epoch (as the latest part of the post-Tertiary), following the suggestion of Forbes (1846, p. 402-403). Le Conte even proposed elevating the Recent to era rank, as the "Psychozoic Era" (1877, p. 114); this proposal was followed by some (Schuchert, 1915, 1924) but did not gain general acceptance. In 1903 the U. S. Geological Survey (1903, p. 27) adopted Recent as an epoch (or series) of the Quaternary period (or system), following the Pleistocene epoch (series), and this usage is still in force. In Europe most geologists and all the official Geological Surveys refer to Recent or its equivalents, Holocene[1], or Postglacial, as an epoch (series). Thus, definition of Recent as an epoch (series) has the force of long-established custom both in the United States and in Europe.

* Publication authorized by the Director, U. S. Geological Survey.

[1] Holocene (meaning "wholly recent", referring to the percentage of living organisms) was originally proposed as a "stage" following the Pleistocene "stage" by the Portuguese committee to the Third International Congress of 1885 (p. 382). It has been widely adopted in Europe as the second and last epoch (series) of the Quaternary period (system).

ARGUMENTS FOR REDUCING THE STATUS OF THE RECENT

Geologists who advocate reducing the Recent in status offer three principal arguments:

1. The climatic argument: The Pleistocene is a glacial epoch, and we are still in this epoch because some glaciers still exist.

2. The time argument: The Recent is too short an interval of time to merit epoch-series status.

3. The rock-stratigraphic argument: The post-Wisconsin rock-stratigraphic record is too limited to justify series status for the Recent.

THE CLIMATIC ARGUMENT

Those advocating the climatic argument think that the boundaries of the Pleistocene should be defined on the basis of what they consider the fundamental characteristic of the epoch: glaciation. The lower boundary of the Pleistocene should coincide with the first onset of glaciation and the upper boundary with the final disappearance of glaciation. According to this viewpoint the Pleistocene epoch still is not ended because glaciers and ice caps exist now in many parts of the world.

The fallacy of this argument is that, although glaciation undoubtedly is an important characteristic of the Pleistocene epoch, neither it nor other manifestations of climatic change have proved to be workable criteria for defining major time-stratigraphic boundaries that are valid inter-regionally. This is well illustrated by the problem of defining the lower boundary of the Pleistocene. In 1948 the 18th International Geological Congress fixed the Pliocene-Pleistocene boundary, following the recommendation of its Temporary Commission on the Pliocene-Pleistocene Boundary. This recommendation was made after extensive consideration of the question by representatives from many countries, including three from the United States. The base of the Calabrian formation (marine) in its type area in Italy, together with the base of its continental equivalent, the Villafranchian of northern Italy, was adopted as the boundary for the following reasons:

1. It is in the historic type section for the Pliocene-Pleistocene, chosen by Lyell.

2. It is a marine section, such as is used in defining nearly all the older period boundaries.

3. It is the most satisfactory faunal boundary, both in the type section and in the marine sections elsewhere in Europe.

4. This faunal boundary also coincides with a widely recognizable rock-stratigraphic break.

Nearly all the geologists on the Temporary Commission believed that it was desirable that the glacial characteristics of the Pleistocene should find expression in its boundaries, but they recognized that this hope can be only partly realized in the practical problem of framing a workable definition of the boundary. The stratigraphic and faunal break in the type Italian section that was selected as the lower boundary marks the beginning of a cooler climate than had prevailed in former (Astian-Plaisancian) times, but it is far older than the onset of Günz (= Nebraskan?) glaciation, and probably ante-

dates even the somewhat dubious Donau glacial beds of pre-Günz age (Eberl, 1930) that formerly were regarded as Pliocene in age. Zeuner (1950 a, b) opposed the majority opinion of the Commission and the Congress on the ground that by the selection of this boundary *much more than half of the Pleistocene would be preglacial.* The Commission and Congress, however, evidently regarded the usefulness of the division, as well as the long years of precedent, as carrying more weight than the desirability of characterizing the Pleistocene (alone among geologic time units) on climatic inference rather than stratigraphy.

Thus the philosophy that the Pleistocene must be defined on the basis of glaciation has already been discarded so far as the lower boundary is concerned.

Nor does there seem to be any valid stratigraphic reason why the Pleistocene need include all episodes of Quaternary glaciation down to today. Stratigraphically it would be entirely feasible to terminate the epoch at an earlier horizon, such as a prior time of extensive deglaciation. Hunt, for instance, has recently proposed (1953) that a widespread unconformity that records the thermal maximum (the altithermal age of Antevs (1948)) in the Rocky Mountain region is a mappable boundary between the Pleistocene and Recent. A distinctive soil formed during this interval in the region is also a possible boundary marker (Richmond, in press; Morrison, in preparation). This interval, incidentally, seems to have been a time of practically complete deglaciation in the cordillera of the western United States and also in most of Canada. Further, taking the boundary at such an unconformity or soil agrees in full with the definition as stated by Lyell in 1863 (p. 5):

> In the Recent we comprehend those deposits in which not only the shells but all the fossil mammalia are of living species.

It has been pointed out by Keilhack (1926, p. 455) that the contrast between the modern fauna and the extinct elk, mammoths, etc., constitutes a faunal difference between Alluvium [Recent] and Diluvium [Pleistocene] as marked as those associated with other series boundaries in the geologic column.

THE TIME ARGUMENT

According to the time argument the Recent is much too short in duration to merit status equivalent to the older epochs. Even in the Cenozoic the shortest epoch, the Pleistocene, is now commonly regarded as having lasted about two million years; in contrast, the Recent probably is between about 10,000 and 4,000 years long (Antevs, 1952, p. 102-105; Deevey, 1953, p. 275-279; Flint, 1953, p. 174; Emiliani, 1955, p. 565, 570; Suess, 1956, p. 365) according to various definitions of the Pleistocene-Recent boundary and various current chronologies of the late Quaternary.

This argument, however, fails to recognize a fundamental fact about geologic time (and time-stratigraphic) classification: time is subdivided on a purely arbitrary, subjective basis, largely for convenience in classification of formations and depending upon the amount of detail recognized in the rock-stratigraphic record. Duration has never been a determining factor in defining

time units, just as thickness has not been in defining formations or other rock-stratigraphic units.

This point is illustrated by the inequality of time divisions in the rest of the geologic column. In general, of course, time divisions become smaller toward the present. This is because the detail of stratigraphic information generally increases in the younger units, because the younger strata tend to be increasingly better preserved and better exposed. But this trend is not invariable. The Jurassic, for instance, is more finely subdivided than the Cretaceous, although it was perhaps only a third as long, merely because Jurassic sections show a greater number of recognizable faunizones than do Cretaceous sections. The brevity of the Pleistocene compared with the Pliocene or Miocene, and the greater number of its subdivisions, is partly because better preservation and fuller exposure give more complete stratigraphic information, and partly because the climatic oscillations that characterized the epoch caused many conspicuous stratigraphic breaks.

For the same reasons, even greater stratigraphic detail is available from Recent deposits, as is illustrated below. Many of these rock-stratigraphic units can be correlated regionally and justify time-stratigraphic subdivisions not only of stage but also of substage rank. Moreover, it should be remembered that archaeologists have established very detailed chronologies that can be tied in to these units and certainly in the future will be still more useful in this way.

THE ROCK-STRATIGRAPHIC ARGUMENT

Some geologists have argued that there is a general lack of deposits that can be consistently differentiated as rock-stratigraphic units younger than those of Pleistocene age. Only a few years ago this argument had more validity than now, for the stratigraphy of Recent deposits actually was poorly known, reflecting the relative lack of attention to these deposits by geologists. Modern stratigraphic studies, however, have disclosed distinctive sequences of Recent deposits in many areas—deposits that can be readily distinguished from Pleistocene deposits on the basis of lithology, interstratified soil profiles, and even fossils. The following paragraphs illustrate the type of detail now available on Recent stratigraphy in a few representative areas of the western United States.

RECENT STRATIGRAPHY IN THE LAKE LAHONTAN AREA

The late Quaternary deposits of the Carson Desert, near Fallon, Nevada, record with unusual sensitivity the climatic history of the northern Great Basin and corresponding changes in lake levels, erosion, sedimentation, and soil development. Figure 1A summarizes this stratigraphy (Morrison, in preparation). The youngest deep-lake sediments of Lake Lahontan are overlain by subaerial deposits, a widespread disconformity, and a moderately developed soil, recording a relatively long interval of complete desiccation of the Carson Desert, and then by sediments of five successive shallow-lake cycles, alternating with those of five desiccation intervals (including the present one.) The five youngest post-Lake Lahontan desiccation intervals were all less pronounced

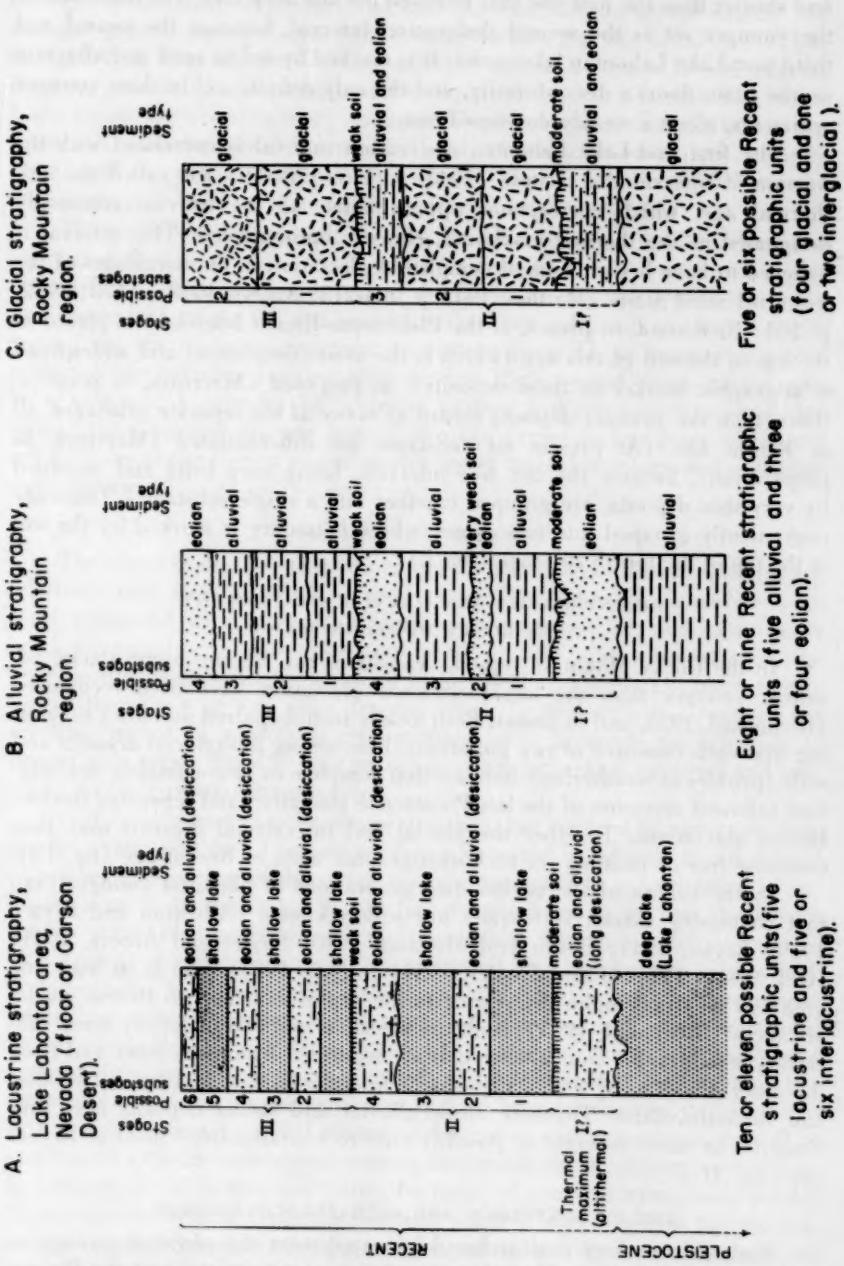

Fig. 1. Recent lake, alluvial and glacial stratigraphy in the western United States, showing possible stages and substages.

and shorter than the first one that followed the last deep lake. The main one of the younger set is the second desiccation interval, between the second and third post-Lake Lahontan lake cycles. It is marked by eolian sand and alluvium on the basin floor, a disconformity, and the only definite soil in these younger sediments, albeit a weakly developed one.

The first post-Lake Lahontan desiccation interval is correlated with the warm-arid interval that Antevs (1948, p. 176, 178-179) has called the altithermal age, which corresponds approximately to the interval commonly designated as the thermal maximum, climatic optimum, etc. This interval is believed to have been a time of complete deglaciation in the mountains of the western United States (Matthes, 1942, p. 204-215; Antevs, 1948, p. 178; 1952, p. 104; Richmond, in press). If the Pleistocene-Recent boundary is placed at the top of the soil of this age (which is the most conspicuous and widespread stratigraphic marker in these deposits), as proposed (Morrison, in preparation), then the younger deposits record as many as ten separate substages, all of Recent age. (At present six substages are differentiated (Morrison, in preparation), because the last five intervals, being very brief and recorded by very thin deposits, are grouped together into a single substage.) These are conveniently grouped into two stages, whose boundary is marked by the soil at the top of the fourth substage (fig. 1A).

RECENT GLACIAL AND ALLUVIAL STRATIGRAPHY IN THE ROCKY MOUNTAIN REGION

In the Rocky Mountain region, deposits of two Recent minor glacial advances younger than the last Pleistocene glaciation have been recognized (Richmond, 1955, and in press). Both locally include paired moraines suggesting that each consisted of two pulsations. Intervening interglacial deposits and soils (profiles of weathering) indicate that complete or near-complete deglaciation followed recession of the last Pleistocene glaciation and separated the two Recent glaciations. Together the glacial and interglacial deposits may thus comprise five or possibly six rock-stratigraphic units of Recent age (fig. 1B).

In the valleys of the region, two generations of alluvium younger than that containing extinct vertebrates are widely known (Albritton and Bryan, 1939; Bryan, 1941; Bryan and McCann, 1940; Sayles and Antevs, 1941; Hack, 1942; Leopold and Snyder, 1951; Judson, 1953), and in at least one area can be stratigraphically related to the deposits of the two Recent glacial advances (Richmond, 1955). Both are divisible into two or locally three subsidiary stratigraphic components, and are separated from each other and from the older alluvium by eolian deposits of interglacial age, and by soil profiles and disconformities. Together, these alluvial and eolian deposits may thus comprise as many as eight or possibly nine rock-stratigraphic units of Recent age (fig. 1C).

THE PALEONTOLOGIC AND ARCHAEOLOGIC RECORDS

Both paleontology and archaeology supplement the physical geology in subdividing Recent deposits and setting them apart from those of the Pleistocene. Freshwater and terrestrial invertebrates, for example, are sensitive to

changes in moisture availability so that the composition of the faunas has changed greatly during Recent time despite the fact that the list of species has changed little if at all. Even within a single valley, successive Recent alluvial deposits commonly contain strikingly different invertebrate faunas. The vertebrate fossils are not sufficiently abundant to record faunal changes within the Recent but they do show differences between the Pleistocene and Recent, partly because of the disappearanc of some species like the mammoth and ground sloth and partly because of evolution in others like the bison.

The archaeological record, likewise, provides means for subdividing and separating Recent deposits. Pottery is an example. It was introduced in the United States about the beginning of the Christian era and provides a ready and convenient means for distinguishing the Recent deposits of the last 2,000 years from the more ancient ones. In various parts of the country two pre-pottery lithic cultures, younger than the late Pleistocene Folsom culture, can be recognized. A considerable series of pottery and non-pottery occupations during the Christian era are known. We can expect to recognize additional subdivisions of the Recent as our knowledge of the paleontology and archaeology improves.

RANK OF THE RECENT

The physical deposits that record the various distinct Recent episodes are relatively thin and restricted in areal extent. The intervals themselves were short compared with intervals of earlier Quaternary time, and exceedingly brief compared with pre-Quaternary geologic time units; nevertheless, the time involved spans much of human history and its details are more clearly legible than those of any more ancient series. To deal with this complex but readable record in acceptable stratigraphic terms demands the retention of Recent as a series. Only so may its subdivisions be treated as stages and substages as their relationships require.

If it be proposed that Recent be retained, but reduced to age-stage rank, the only valid and useful reason for doing so would be because the likelihood of further subdivision of the series (as now recognized) is considered so improbable as to be negligible. But it has already been found that the Recent can be subdivided usefully, and the probability that such subdivisions will ultimately be greatly refined has been expressed by many workers in this field. Thus reducing the rank of the Recent would make it necessary to recognize probably two categories of time-stratigraphic terms subordinate to *age-stage*. By retaining Recent as a series, these two categories can be accommodated without altering the present accepted outline of classification. Even if two categories are never needed, no harm is done by retaining the *status quo;* if they become necessary and if Recent is reduced to age-stage status, we will be confronted with the necessity of coining new words for a category subordinate to substage, or of further restricting the usage of common terms badly needed in non-specific senses. From the practical point of view (at least for the continental United States), nothing would be gained by reducing the Recent from its present epoch-series status. The international acceptance of the lower boundary as agreed upon in 1948 by the International Congress has already

foreclosed the use of "Pleistocene" as a synonym of "Ice Age." The fact that the present is still a glacial time can be expressed quite independently of the formal stratigraphic nomenclature, just as it is understood by all that chalk deposits are not confined to the Cretaceous.

Calvin (1898, p. 355) put the case for conservatism very well: "Any classification is arbitrary at best. A dozen or more equally good schemes might be proposed, but we should adopt and strengthen as far as possible that which is in most general use, notwithstanding the fact that it might be improved in many respects." Indeed, as Wolstedt remarked (1950, p. 113), apparently no serious consideration has been given in Europe or elsewhere to the proposal originating in the United States that the Pleistocene be extended down to the present.

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COMMENTS ON SOME STRATIGRAPHIC TERMS

O. H. SCHINDEWOLF

ABSTRACT. The methods, concepts, and terms of stratigraphic geology are at present being vigorously discussed from standpoints often widely diverging. Arkell in a recently published paper has strongly emphasized the differences separating him from my former interpretations. In the following pages I am trying to reach a better agreement by critically examining such concepts as formations, zones, stages, and ages and by removing some misunderstandings.

INTRODUCTION

During the 1956 session of the International Geological Congress in Mexico City, a Subcommission on Stratigraphic Terminology aimed at clarifying the methods, concepts and terminology of stratigraphy. Strangely enough, there is at present much dissent about the theoretical interpretation of stratigraphy and its principles, though in practice stratigraphic work has been done for about two centuries (in fact, the year 1956 may be regarded as the two hundredth anniversary of stratigraphy, since, in his work of 1756, J. G. Lehmann seems to have been the first who described, figured and compared geological sections). Apparently, the theoretical understanding of what we are doing has lagged behind the common sense and practical insight of our great old masters of geology.

The discussions of the above-mentioned subcommission were ably prepared by its secretary, H. D. Hedberg. He not only published a basic study on "Procedure and terminology in stratigraphic classification" (Hedberg, 1954), but also sent out a number of voluminous questionnaires to ask the opinions held by the members of the subcommission. Stimulated by the wish expressed by Hedberg for critical comments, I, myself, wrote a short paper from a somewhat opposed point of view (Schindewolf, 1955).

Both these papers have now been severely criticized by W. J. Arkell (1956) in volume 254 of this Journal. I am glad to see that, in many points, we fully concur, though in others there is a wide divergence of opinion partly due to apparent misconceptions of Arkell. By correcting them, and by further reasoning, I will try to reach a better reconciliation of our standpoints.

STRATIGRAPHY, BIOSTRATIGRAPHY, AND PROSTRATIGRAPHY

The basic problem of stratigraphy is a very simple one. Its task can be defined as aiming at a classification of the existing sedimentary rocks (and associated rock bodies) in their historical, chronological order, including, of course, a determination of all the synchronous equivalents developed in different facies. The time concept is, therefore, invariably linked with stratigraphy in its proper sense.

The sedimentary rocks, by themselves however, do not yield any specific time marks, setting aside the old law of superposition, which can provide relative age indications only in a restricted manner and which is unfit for age correlations. Moreover, it may be misleading in some cases: the beds in a section may be overturned or, owing to a hidden thrust plane, older beds may overlie younger ones.

The only chronometric scale applicable in geological history for the stratigraphic classification of rocks and for dating geological events exactly is furnished by the fossils. Owing to the irreversibility of evolution, they offer an unambiguous time scale for relative age determinations and for world-wide correlations of rocks. The absolute age determinations are by far too inaccurate for stratigraphical purposes, and all the other methods lack more than local or restricted regional applicability.

If there is general consent about these basic facts, then a "time-stratigraphy" and a "biostratigraphy", as advocated by Hedberg and others, seem to be meaningless. In my opinion, they are mere synonyms of stratigraphy in its proper sense, since the time concept can by no means be separated from stratigraphy, and since a refined stratigraphy of more than local validity can only be based on life (bios).

On the other hand, "lithostratigraphy", to my mind, is no genuine and definite stratigraphy at all, if, by the definition given by Hedberg (though apparently not always in practice), the time concept is explicitly eliminated. It can only describe the rocks with regard to their petrographical properties and their local superposition, without any means of determining their age and their time relations to other rocks even in the vicinity. But stratigraphy is more than merely describing, distinguishing and denominating rock units without any aim of dating them. Labeling each separate rock category with a local name cannot bring any definite scientific insight, if, by neglecting the time factor, every principle of linking the differently named formations exactly is lacking.

In my previous paper (1955, p. 28), I compared lithostratigraphy with a primitive stage of biological morphology which would not possess the methodic concept of homology. Such a merely descriptive morphology would attribute separate names to all the organs differing in shape from form to form, though, in reality, they are homologous and have to bear the same designations. What homology is in morphology, the time concept is in stratigraphy, forming its characteristic methodic tool.

Thus, to my mind, "lithostratigraphy" is only a provisional, tentative step towards the final chronological determination of rocks, which alone deserves the title of stratigraphy. I, therefore, have called it "rather disparagingly" (Arkell, 1956, p. 462) "Prostratigraphie" (prostratigraphy), mainly to make quite clear the logical situation.

Arkell has misinterpreted this term as if it designated something superfluous or valueless. But this is far from my own meaning. "Lithostratigraphy" is indispensable as a first approach towards a definite stratigraphic interpretation of a given section, and there may be occasional cases where analysis cannot be carried on beyond this first procedure. But, for the reasons mentioned, this, by itself, is no proper stratigraphy.

If this is generally recognized, the term prostratigraphy may be dropped. It only makes explicit that "lithostratigraphy" cannot be an autonomic stratigraphic method, but only stratigraphic propaedeutics. I hope that Arkell, too, will realize this, especially if he bears in mind that his concept of formations is fundamentally different from that defined in a strictly lithostratigraphic

manner by Hedberg and by the American Commission on Stratigraphic Nomenclature.

FORMATIONS

The term "Formation" has a different meaning in German geological literature. I had referred to this difference from English-language literature (1955, p. 26); in German use, the term "Formation" is employed for what in other countries is called "system" (or in French "terrain"). Since this is fairly generally known, I am somewhat surprised that Arkell, so very well acquainted with German literature, did not notice this difference. He has endeavored to teach me that it does not exist and that my remarks are baseless.

The main work by A. Oppel bears the title: "Die Juraformation Englands, Frankreichs und des südwestlichen Deutschlands", and up till now, we have always spoken of a Devon-Formation, Jura-Formation, etc. We follow, in this respect, the original interpretation of the term Formation by the old German authors at about the end of the eighteenth century. Under this designation, they subsumed a number of successive beds, layers and rock members forming a higher unit, which they conceived as representing a period in the history of the earth. (This was much prior to the introduction of the "system" by R. I. Murchison in 1835).

We are, however, not accustomed to use the term, Formation, in the sense applied to it in English-written literature. We speak, as Arkell correctly states, of Gigas-*Schichten*, Heersumer *Schichten* and so on. Thus, it is not quite exact, when Arkell (1956, p. 460) translates Oppel's "Bildungen" as "formations"; Oppel would not have used the term in that signification.

These different meanings of the same word are deplorable, and therefore, I have recommended (1955, p. 31) that the term, Formation, in that specific German sense, though abundantly justified by priority, should be abandoned in favor of "system".

ZONES

In my previous papers (1944, 1950, 1955), I had interpreted the Oppelian zone as a time-term designating the interval during which the beds containing certain index fossils were deposited. This seems to be the prevalent use in German literature (R. Brinkmann, E. Dacqué, K. Fiege, H. Salfeld, R. Wedekind, and many others). Arkell, however, advocates an opposite point of view: the zone being a stratigraphic term. By lengthy quotations from Oppel's works he tries to show, "once for all", that the different explanation "is a figment of Prof. Schindewolf's imagination". This may be true, but I must confess that the argumentation by Arkell does not fully convince me. I am, thus, not so sure that he is justified in his claim of possessing the sole truth in this matter.

So far as I can judge, Oppel's "ideales Profil, dessen Glieder gleichen Alters in den verschiedenen Gegenden immer wieder durch dieselben Arten charakterisirt werden" may never be conceived as a concrete section with tangible beds of any thickness and lithology *containing* a certain assemblage

of fossils. To my mind, it can only be interpreted as an abstract scheme of ideal chronologic units *characterized* by certain index fossils.

If Oppel, in another instance cited by Arkell, states that the presence of a certain ammonite zone is indicated by spongy limestones, this, again, seems to show that he conceived the zone in a chronologic sense. If this ammonite zone were understood as a stratigraphic unit, spatially restricted to the distribution of the beds with the typical ammonites, it could not very well be indicated by spongy limestones or other rocks without the index ammonites. In other places, Oppel is writing of zones overlying or underlying each other. This may be a somewhat inexact wording not conclusive for the one or the other interpretation.

I will not be as apodictic as Arkell and claim that my interpretation is the only correct one. I have rather the impression that Oppel's sentences are somewhat ambiguous and that he did not perform such fine distinctions as we do today. It is significant enough that he desisted from giving a clear-cut definition of his zone concept. Oppel died as a premature genius in young years; we cannot ask him what he really meant. His writings, however, are apt to be interpreted in the one sense as well as the other.

For the sake of amicable arrangement, I will not insist on my interpretation. If the prevalent use in world literature is that the zone represents a bed or band of rocks characterized by certain fossils, then I will agree with such a definition, which should be fixed by international convention.

Only in this latter sense, it is justified to ascribe to the zone a regional restriction. If, however, in my former papers, I defined the zone as a time concept, I had to deny its restricted distribution, for time is ubiquitous, not local. Fortunately, there is a method of recognizing the time equivalents of any given ammonite occurrence, even if the beds concerned are developed in a widely different facies and the index ammonites are lacking. Instead of the stratigraphically ruling forms (i.e. the ammonites in this case), we use representatives of other animal groups (e.g. brachiopods, ostracods, foraminifers, etc.) which are present in the second facies, and which may be found eventually in both facies or, at least, in any transitional beds connecting the two facial realms.

In former papers, I have spoken of a main-stratigraphy or "orthostratigraphy" that, in such cases, is replaced by a "parastratigraphy", based on fossils other than those governing the prevalent orthostratigraphy. The remarks by Arkell (1951, p. 303) show that he completely misunderstood the meaning of this distinction, which seems to me self-evident. It aimed to emphasize that among a score of available stratigraphic possibilities, based on different groups of animals or plants, *one* has to be selected as the most suitable and effective one. In the Cambrian, this standard stratigraphy is founded on the trilobites; in the Silurian, on the graptolites; in the Mesozoic, on the ammonites. If, however, these prevailing animal groups are lacking for any reason, one must substitute other forms at hand: brachiopods, corals, ostracods, etc. The stratigraphical divisions yielded by them have to be correlated with our standard stratigraphic units and have to be subordinated to them.

This is an old and long-used principle, which I only tried to accentuate by introducing the two mentioned terms. Whether they are suitable may be questionable. But it is decidedly false for Arkell (1951, p. 303) to ascribe to me that naïve interpretation: "Orthochronology is chronology based on fossils of chronological value; parachronology is chronology based on fossils, in the circumstances, chronologically useless".

When Arkell asserts that the employment of the words "correlation" and to "correlate" in British literature is legitimate, I have no objections. Of course, I was concerned first of all with the German application of the equivalent terms "Korrelation" and "korrelieren". They are of Latin derivation but have undergone some deviation from their original meaning in our geological terminology. Now, indeed, the language is flexible, as Arkell said, but I must insist that "korrelieren" is a very bad German expression for which I would not like to find wider distribution.

STAGES AND AGES

Arkell is right that my classification of stage (or the corresponding German Stufe) as a stratigraphical equivalent of the zone, conceived as a time-term, was unhappy. Of course, I was aware of the fact that the stage denotes a wider unit than the zone. The concept of Stufe has been employed in German literature especially by R. Wedekind. Both the zone and the Stufe were used by him in a strictly chronological sense, and not as stratigraphical terms. Concerning the zone, I followed him, since this interpretation seemed to me in accordance with Oppel's intention. The stage or Stufe, however, has been fixed by former geological congresses in the sense of a stratigraphical term, and I saw no reason to deviate from that decision.

I, myself, felt that my parallel was inconsistent, and so I replaced it, independently, in my recommendation to the Subcommission on Stratigraphic Terminology by another one, linking the stage (Stufe) as the stratigraphical counterpart with the time-term age (Alter) (see Circular No. 5 of that Commission from August 1956). I hope, thus far, to be in accordance with Arkell.

On the other hand, I cannot quite agree with his statement that these "age-terms serve no useful purpose" and "that they are strictly limited by the area of occurrence of the name-fauna". I see no hindrance to speak of a Cardioceras-Age or Cardioceratan even outside of the northern hemisphere in regions where *Cardioceras* does not exist and is replaced by any other ammonites. I must repeat that time and our time-units are universal. We may speak of an age of reptiles or of ammonites, even where reptiles and ammonites are lacking.

Furthermore, it is an often-stated fact that our stratigraphical column and our delimitation of geological periods would show a very different face if they had originally been based on the evolution of rocks in India or in regions of the southern hemisphere, e.g. Africa or South America. Nevertheless, it is indispensable to fix a standard classification, and, for mutual understanding, to apply it also to continents with an entirely different geological evolution. The Permian period is ubiquitous, be it developed in any facies whatever or be it in some regions not even represented by rocks and fossils

at all. Therefore, to my mind, the Cardioceratan age can be taken as a time abstraction, as well as the Oxfordian or the Callovian.

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PETROLOGY OF QUATERNARY VOLCANICS OF THE MOUNT GARIBALDI MAP-AREA, SOUTHWESTERN BRITISH COLUMBIA

W. H. MATHEWS

ABSTRACT. The petrology of a suite of volcanic rocks having limited age range and areal distribution but wide compositional differences is described. The mineralogy shows remarkably little relationship to bulk composition of the rocks. Cooling histories, particularly as influencing mineral zoning and degree of crystallization, and pressure histories are important in determining what minerals are present and what is the composition of these minerals. Rock classification based on chemical composition is hence more suited for correlation of these rocks than is one based on mineralogic composition.

Petrologic characters and the volumetric distribution of rock types suggests the differentiation of a basaltic magma contaminated by granitic material, followed by gravitative selection during extrusion.

INTRODUCTION

Geologic mapping in the Mount Garibaldi map-area, in the southern Coast Mountains of British Columbia (fig. 1), discloses a varied group of volcanic rocks postdating the last major uplift (Pliocene?). Lavas and pyroclastics of this group range from basalt to dacite, and an associated intrusive consists of rhyodacite glass. No less than 30 distinct volcanic centers exist in the 14 by 25-mile map-area. Information obtained by field mapping permits a more or less reliable estimate of the volume of rock associated with each center.

Fig. 1. Index map of the Mount Garibaldi map-area (small rectangle) showing distribution of Quaternary volcanic rocks in black.

Petrologic study of these volcanic rocks has been made to establish

(1) the range in bulk composition
(2) the volumes of rock in each of several compositional groups, and
(3) the relationships between bulk composition and composition of constituent minerals.

Field mapping was carried on during the summers of 1946 and 1947, and petrologic studies were undertaken mainly during the succeeding two winters. Approximately 150 hand specimens were available for study from which 50 thin sections and 16 rock analyses were obtained. Artificial fusions of the chemically analysed rocks displayed a close relationship between refractive index and composition (Mathews, 1951) and this relationship was used to determine the approximate composition of the remainder of the hand specimens. Mineral composition was determined by optical methods as outlined below.

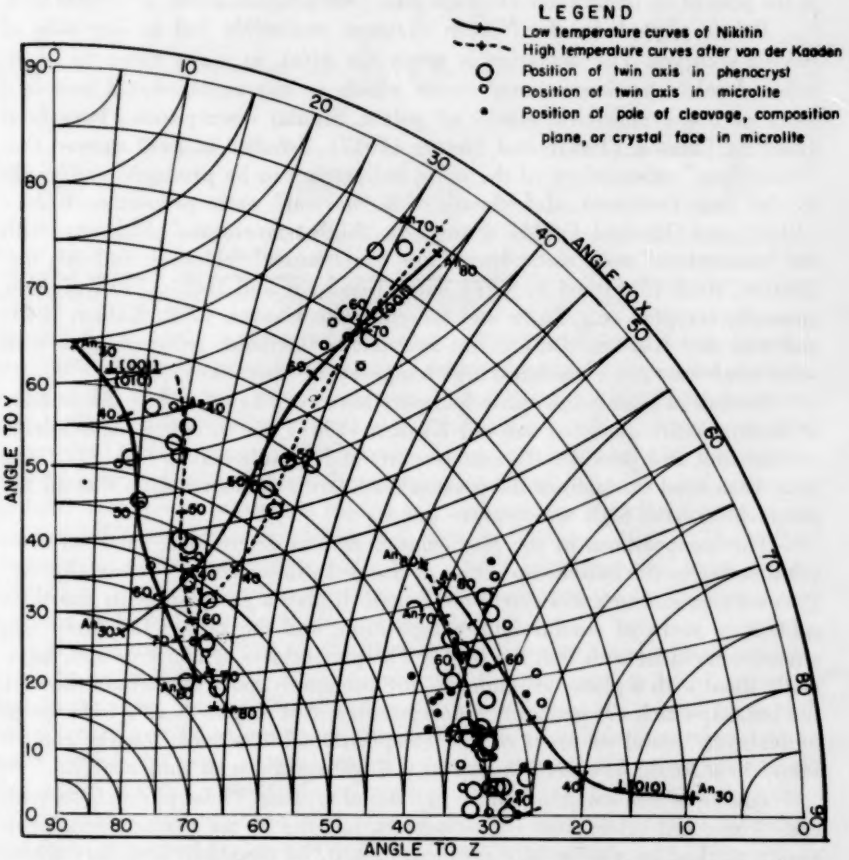

Fig. 2. Optic orientation of twin axes in plagioclases of the Quaternary volcanic rocks.

MINERALOGY

Plagioclases.—Determination of plagioclases in thin section has been carraied out as far as possible by the Fedoroff method as outlined by Nikitin (1936) and described in English, with minor modifications, by Turner (1947). In this method the relationship between the optic indicatrix of a twin lamella and the twin axis is determined by universal stage, and the twin axis is identified by its angular relationship to some other composition plane or cleavage in the same crystal. The data are plotted on an appropriate diagram (fig. 2) and compared with the standard curves provided by Nikitin. Under favorable conditions the positions of indicatrix and twin axis can be established within a very few degrees. However, strong zoning in some phenocrysts reduces accuracy, and in microlites the overlapping of twin lamellae or of crystal and adjoining, clouded groundmass limit the possible observations. In many microlites only two optic directions could be measured and these had to be related to the pole of an identifiable cleavage plane, composition plane, or crystal face.

Points plotted on the Nikitin diagram commonly fall to one side of Nikitin's curves. The deviation is much too great, in many cases, to be attributed solely to observational errors which, in any event, should lead to a more or less symmetrical scatter of points. Similar discrepancies have been noted by Larsson (1940) and Turner (1947). Scholler in 1942 showed that "anomalous" orientations of the optic indicatrix can be produced artificially by the heat treatment of feldspars with "normal" optic properties. Köhler (1942) and Oftedahl (1944) distinguish "high temperature" feldspars, with the "anomalous" optic data, from "low temperature" feldspars, and this distinction, since confirmed by x-ray data (Goodyear and Duffin, 1955), is now generally accepted (e.g. Tuttle and Bowen, 1950; Chayes, 1952; Köhler, 1949; and van der Kaaden, 1951). The anomalous Garibaldi feldspars can, with some confidence, be considered high temperature varieties.

Studies of high temperature feldspars have been hampered by the scarcity of determinative data, but van der Kaaden (1951) has compiled the available information and provides the counterparts of Nikitin's curves (fig. 2). These have been used throughout the present work, with the assumption that *all* the plagioclases have high temperature optics.

The compositions of the plagioclases, as thus determined, show no close relationship to the bulk composition of the rocks in which they occur (fig. 3). Phenocrysts, and to a less extent microlites, display a great range in anorthite content in rocks of similar bulk composition, and the ranges for dacite are almost coincident with that for basalt. The plagioclases of many dacites, especially those with a glassy groundmass, are commonly more calcic than those of the basalts, which are generally holocrystalline. Not only is there a wide range of feldspars found in rocks of one composition, but a great variety can be found in a single flow, and indeed in a single specimen or thin section.

Zoning is also common within individual crystals. Those phenocrysts with sodic cores and calcic rims are xenocrysts, judging by the reaction zone commonly marked by a series of glass inclusions, or by the sharp boundary which generally separates the inner and outer parts of the crystal. They may be

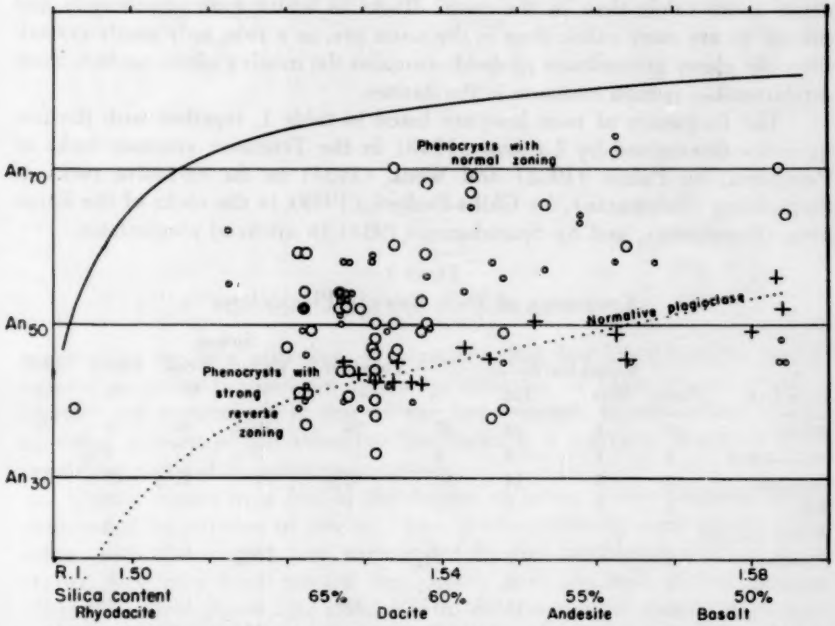

Fig. 3. Relationships in the Quaternary volcanic rocks between plagioclase phenocrysts (large circles), microlites (small circles), normative plagioclase (crosses), and the refractive index of the fused rocks in which they occur. Solid line: composition of first crystals from a melt with the composition of the normative plagioclase.

derived from partly assimilated fragments of quartz diorite, for their composition and dimensions approximate the feldspar grains of the earlier plutonic rocks of the map-area. The phenocrysts with normal zoning or whose cores are similar in composition to their rims probably crystallized from a magma represented by the present volcanic rocks. These are compared with the normative feldspar (calculated from the chemical analyses) (fig. 3), and with the feldspar that would crystallize from a pure plagioclase melt having the composition of this normative plagioclase feldspar (Bowen, 1913). The stable phenocrysts prove to be more sodic than would be anticipated from a consideration of the plagioclase cooling curves alone, just as in the San Juan volcanic rocks of Colorado (Larsen et al., 1938).

The outermost shell of all the phenocrysts, irrespective of type of zoning, tends to be uniform in thickness from grain to grain, and to be of approximately the same composition or range of composition as the microlites in the same rock. Even these outermost shells, like the microlites themselves, show some zoning, being in general somewhat more sodic at the outer surface of the crystal than at a point few microns or tens of microns toward the core. They evidently represent the layer crystallized after the lava has been extruded and thus correspond to the crystals of the second generation.

The microlites of only a few of the rocks are more sodic that the normative plagioclase of the same rock; presumably they compensate for pheno-

crysts more calcic than in the norm. Rocks in which both phenocrysts and microlites are more calcic than in the norm are, as a rule, only partly crystalline; the glassy groundmass probably contains the missing albite content. Such a relationship is most common in the dacites.

The frequency of twin laws are listed in table 1, together with the frequencies determined by Larsson (1940) in the Tronador volcanic rocks of Patagonia, by Paliuc (1932) and Wenk (1933) in the extrusive rocks of Siebenbürg (Roumania), by Ghika-Budesti (1939) in the rocks of the Banat area (Roumania), and by Spaenhauer (1933) in artificial plagioclases.

TABLE 1

Frequency of Twin Laws in Plagioclases

	Mount Garibaldi				Tronador	Sieben. Paliuc	Wenk	Banat	Artif.
Twin Law	Phen.	Micr.	Tot.	%	%	%	%	%	%
Albite	20	8	28	37	24	49½	45	26	28
Mannebach	2	1	3	4	—	5½	7	9½	—
Carlsbad	9	5	14	18	38½	14	17½	26	43
Ala	3	2	5	7	1	—	—	—	—
Acline and/or Pericline	2	—	2	2½	2	24½	13	—	—
Albite-Ala	2	—	2	2½	2	—	—	—	—
Albite-Carls.	10	5	15	20	32½	4	15	22½	29
Baveno	—	—	—	—	—	1½	4½	—	—
Ambiguous	6	1	7	9	—	—	—	8*	—
Total Number Determined	54	22	76	—	95	431	149	53	68

* Described as "others"

The determinations of the Mount Garibaldi feldspars indicate that the most common twins are Albite, Albite-Carlsbad, and Carlsbad, in that order, and that there is little difference in the relative frequency of twin laws between phenocrysts and microlites. The results are in accord with those of Paliuc and Wenk who also investigated rocks in which intermediate plagioclases predominated. Larsson (1940, p. 373) indicates that the frequency of twin laws is a function of composition and that in the calcic plagioclases Carlsbad twinning predominates, but no such relationship is recognized in plagioclases from the Mount Garibaldi map-area.

The normative composition of feldspar in the chemically analysed rocks is illustrated diagrammatically in figure 4.

Potash feldspar.—No potash feldspars are known in the volcanic rocks of the map area.

Silica minerals.—Only two silica minerals have been recognized in thin sections of the volcanic rocks: Cristobalite and quartz.

Cristobalite occurs both in cavities and in the groundmass of holocrystalline dacites. It characteristically displays a series of overlapping plates "like tiles on a roof" (MacGregor, 1938) or the surface pattern of a golf ball (Howel Williams, pers. comm.) given it by cleavage and twinning (?). This

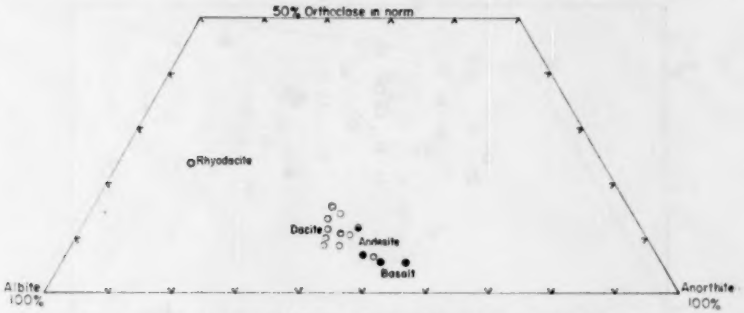

Fig. 4. Normative feldspars in the Quaternary volcanic rocks.

appearance, together with distinct negative relief, low birefringence, and an uniaxial negative interference figure recognizable in some grains, serves to identify the mineral. It is one of the last minerals to crystallize. Though abundant in some of the holocrystalline rocks, it is scarce or absent in merocrystalline rocks of the same composition.

Quartz occurs in a few of the dacites as large, widely-scattered grains, surrounded by coronas of augite. These grains evidently were not in equilibrium with the magma but, surrounded by the continuous shell of augite grains, they may have reacted very slowly with the melt. Strain shadows, visible in a few grains, are attributed to deformation of quartz prior to incorporation in the magma. These quartz xenocrysts may have been derived by partial assimilation of quartz diorite. The presence of accompanying feldspar xenocrysts tends to confirm this suggestion, but inclusions of quartz diorite as such are rare and show no notable signs of assimilation. In partly assimilated inclusions of arkose in one dacite, though the cement has been converted to glass, the aggregate retains its continuity. In a more advanced stage of vitrification, inclusions of both arkose and quartz diorite might have become disintegrated into isolated xenocrysts of quartz and feldspar.

Orthopyroxenes.—Orthopyroxene is the most common of the mafic minerals occurring as phenocrysts in many dacites and a few andesites, and as a groundmass mineral in nearly all dacites. In some rocks it is the only mafic mineral present; in others it is accompanied by amphibole, by augite, or by augite and olivine.

Composition of orthopyroxenes was determined using universal stage measurements of 2V and the optic data of Hess and Phillips (1940). These determinations show the orthopyroxenes to range from En_{60} to En_{94} and to average En_{75}. The magnesia content shows little relationship to the composition of the rock (fig. 5) or to the presence or absence of other mafic minerals. The MgO/FeO ratios are, with one exception, higher than in associated augites, unlike the relationship in Skaergaard pyroxenes (Muir, 1951, p. 709-711). The MgO/FeO ratio tends also to be higher in the orthopyroxenes than in the rock as a whole, even in those rocks containing no other ferromagnesian mineral (fig. 5). In these rocks the ferrous iron may be concentrated in the glassy groundmass.

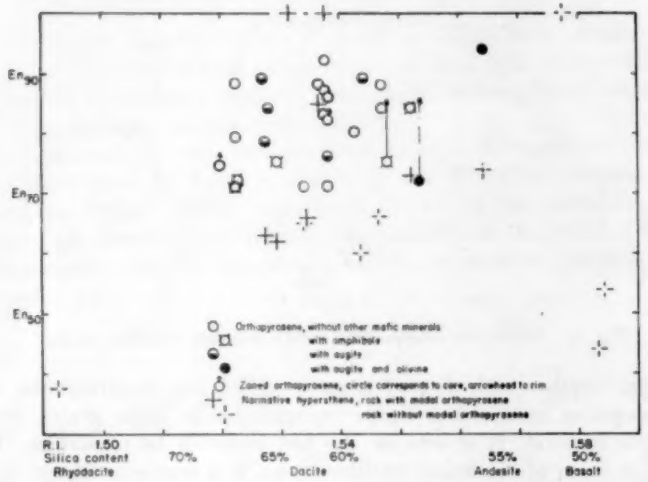

Fig. 5. Relationship betwen modal and normative composition of orthopyroxenes of the Quaternary volcanic rocks and the refractive index of the rocks in which they occur.

Reverse zoning of orthopyroxene, in which the core is distinctly richer in iron than the rim, has been observed in several sections but most of the orthopyroxenes are nearly uniform in composition throughout. None has been observed which becomes markedly richer in iron from core to rim.

Amphibole.—Both green and brown hornblende are present in many dacites and are generally but not invariably accompanied by orthopyroxene.

Oxyhornblende occurs with normal hornblendes in a few rocks. Resorption rims are common on both hornblende and oxyhornblende and in many rocks the amphibole has become completely converted to a pseudomorph clouded with black opaque dust. In one specimen an aggregate of orthopyroxene, black opaque minerals, and plagioclase forms such a pseudomorph after amphibole. In many sections gradations can be found between black opaque pseudomorphs and a murky pyroxene.

The relationships of green and brown hornblende to one another and to oxyhornblende has been discussed by MacGregor (1938, p. 51-53) who suggests that the green hornblende, the typical form in deep seated magmas, is converted by 'auto-oxidation' to brown hornblende and oxyhornblende by loss of hydrogen (or water) under reduced confining pressure though at elevated temperatures. The black resorption rims and pseudomorphs of oxyhornblende, of brown hornblende, and, less commonly, of green hornblende, he likewise attributes (p. 54-5) to loss of confining pressure at high temperature.

Augite.—Augite is present in all basalts and andesites and in a few dacites but does not occur with amphibole. It generally forms glomeroporphyritic clusters of relatively small, twinned or untwinned grains. An ophitic texture occurs in one basalt flow.

The compositions of augites in the volcanic rocks, indicated diagrammatically in figure 6, are based on observations of refractive index of cleavage

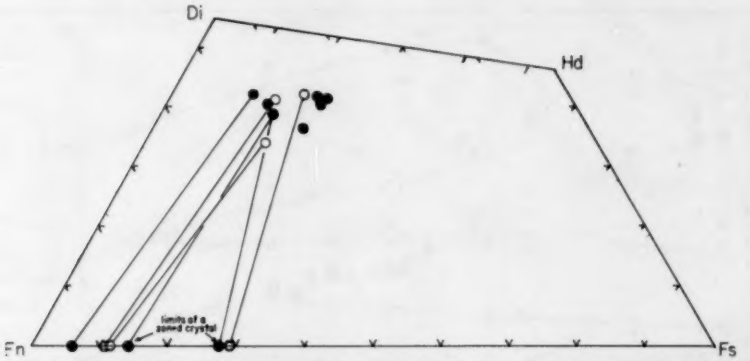

Fig. 6. Composition of augites and accompanying hypersthenes of the Quaternary volcanic rocks. Solid circles: with accompanying olivine of composition Fo_{8-18}; open circles: no accompanying olivine.

flakes together with universal stage measurements of 2V, using the optic data of Wager and Deer (1939). As indicated by Tomita (1934) and affirmed by Wager and Deer, the approximate value of the refractive index γ, used in the optic data, can be obtained by adding 0.004 to the maximum index of a grain resting on a cleavage face. Only a few of the grains in a powdered sample of augite are so oriented and only those showing a second cleavage perpendicular to the slide and inclined extinction can be used for the determination. To further simplify determinations, the angle of rotation necessary to bring the index of the grain into exact coincidence with the index of the immersion oil was measured. The maximum index of the section, γ', was then evaluated, assuming the range of indices in that section (i.e. $\gamma' - \alpha'$) to be 0.020, the average value obtained by Tomita. A chart prepared by Emmons (1943, pl. 10) can be used to determine this maximum index given the range in indices (i.e. 0.020 in this case), the index of the oil, and the rotation.

Olivine.—Olivine occurs in basalts and andesites as large crystals, originally euhedral but now generally embayed and partly resorbed, it is not known in any dacites. It does not occur as a groundmass mineral in the more basic volcanic rocks. Its composition, as determined from universal stage measurements of 2V and the optic data quoted by Winchell (1933), is between Fo_8 and Fo_{18}.

CHEMICAL COMPOSITION

The chemical compositions of 16 samples from the rocks of the Garibaldi group are given in table 2. Variation diagrams, in which are indicated the relationships of silica content to the content of other oxides (fig. 7) and to normative minerals (fig. 8) are plotted from these analyses. A close relationship between refractive indices of fusion products of these rocks and their chemical analyses (Mathews, 1951) are also indicated (fig. 7). In the two variation diagrams, an almost complete sequence can be recognized between the basalts at one end and the dacites, if not the rhyodacite, at the other. Only a few anomalous analyses stand out, notably nos. 12 and 14 in which lime and magnesia contents are unusually high and alumina contents correspond-

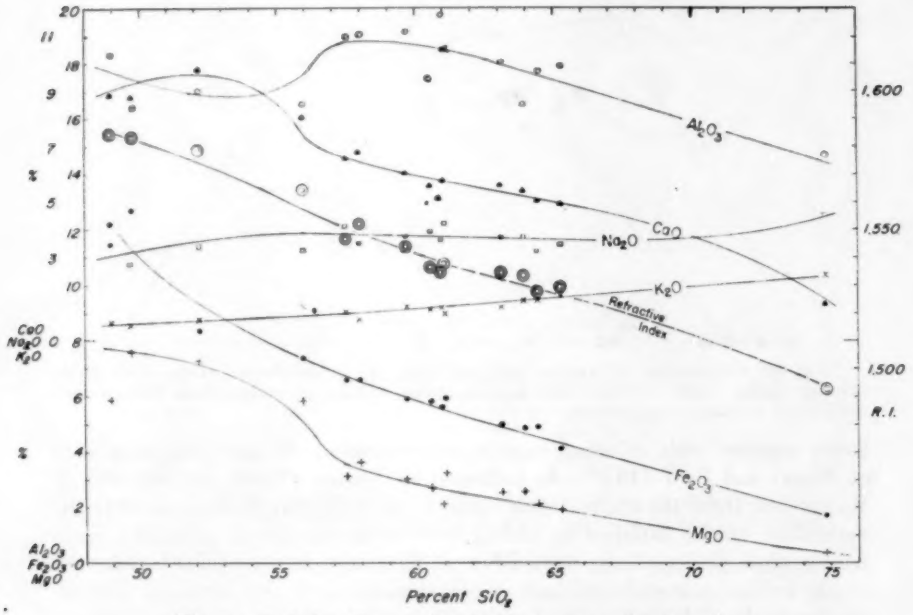

Fig. 7. Variation diagram for the Quaternary volcanic rocks.

ingly low, compared with rocks with similar silica contents. Though such a departure might be produced by accumulation of unusually great amounts of augite in the crystallizing magma and by the resulting displacement of feldspars, the two rocks in question are not exceptionally rich in augite phenocrysts, and if the magma did become overcharged with lime and magnesia by such an accumulation of augite crystals, these must have subsequently become remelted.

The main chemical characteristics of the Quaternary volcanic rocks of the map-area are the low contents of potash and iron and the high content of alumina and lime relative to the average igneous rocks of similar silica contents. The variation diagram for the Garibaldi extrusives shows a much closer correspondence to the diagrams for the volcanic rocks of the High Cascades (Williams, 1932), and of Lassen Peak, than to those for the lavas east of the Cascade Range (Williams, 1935). The high lime and low potash contents are reflected in the unusually high alkali-lime index (Peacock, 1931) of 64.0. In this and in many other respects, the variation curves for the Garibaldi volcanic rocks compare closely with those for the older plutonic rocks of the Mount Garibaldi map-area, the only significant differences being in the slightly higher alumina content and the slightly lower lime content of the later rocks.

ROCK CLASSIFICATION

Nomenclature of the volcanic rocks presents the usual difficulties. Mineralogical composition of the lavas, which is clearly influenced by cooling histories, fails as a reliable basis for classification. Free silica is present as

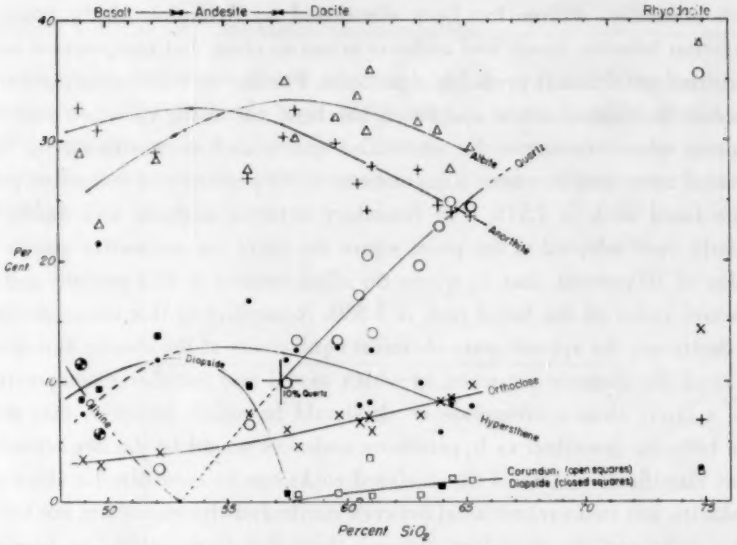

Fig. 8. Variation diagram for normative minerals of the Quaternary volcanic rocks.

cristobalite in many of the holocrystalline rocks but hypocrystalline rocks of similar bulk composition may contain no visible silica mineral. The central part of a single flow might, therefore, be classed as dacite on the basis of the cristobalite content, but the margin of the same flow might equally well be classed as andesite by the absence of visible silica mineral.

Plagioclases, both phenocrysts and microlites, show no close relationship to bulk composition. The MgO/FeO ratio of olivine and hypersthene similarly lacks close correlation with rock composition (fig. 5). Olivine crystals in a more or less advanced state of resorption are present in rocks containing normative quartz. Quartz crystals present in still more acid rocks were not in equilibrium with the magma in which they occurred judging from their augite coronas. The slavish use of any system of mineralogical classification would lead to confusion if applied to these rocks.

The normative rather than the modal composition provides a more consistent basis for classification, although here, too, difficulties arise. Oxidation of a basalt, for example, can alter the normative composition of the rock, giving rise to quartz and hematite in place of olivine and hypersthene. Cost limits the number of analyses than can be made. However, the refractive index of the glass formed by fusing rock samples (Mathews, 1951) offers a reasonably reliable, rapid, and inexpensive method of rock determination once the relationship between composition and refractive index has been established for the suite of rocks in question.

The classification of even those volcanic rocks for which chemical analyses are available is, however, not well established. It may be conceded that those rocks of calc-alkaline suites which are undersaturated, as shown by the presence of normative olivine, can be classed as basalts. For those rocks in

which normative olivine has been eliminated by oxidation of the iron, the distinction between basalt and andesite is not so clear, but composition in the unoxidized condition is probably significant. For the rock of the map-area, the boundary between andesite and basalt has been set on the variation curves at the point where the curves for normative quartz and normative olivine have the value zero, that is where silica content is 53 percent and refractive index of the fused rock is 1.575. The boundary between andesite and dacite has similarly been adopted at the point where the curve for normative quartz has a value of 10 percent, that is, where the silica content is 57.2 percent and the refractive index of the fused rock is 1.550. According to this usage, andesite and dacite are the approximate chemical equivalents of the diorite and quartz diorite of the plutonic sequence, in which modal and normative compositions show a fairly close correspondence. It should be noted, however, that many rocks hitherto described as hypersthene andesites would be dacites according to this classification. One of the analysed rocks can be considered a silica-rich rhyodacite, but rocks transitional between dacite and rhyodacite are not known in this suite and the boundary between these two types cannot be precisely defined in terms of silica content or refractive index.

ORIGIN OF THE MAGMAS

Two distinct modes of origin are suggested for the volcanic magmas of the map-area: (1) that they evolved by the differentiation of a single basaltic parent magma, or (2) that they were derived by the refusion of quartz diorites in the presence of a basic intrusive. Following the first hypothesis, the basalts of Daisy Lake (analyses 15 and 16) could represent the parent magma, and the more siliceous andesites, dacites, and rhyodacite could be its differentiates. In accord with this hypothesis is the almost continuous transition in rock types from basalt to dacite in the Garibaldi group as a whole, and the similarities in the basalt and andesite emitted from the Cinder Cone at about the same time (analyses 13 and 16). The anomalous lime and magnesia-rich lavas (analyses 12 and 14) might, as noted above, be explained by a somewhat involved process of concentration and refusion. The rhyodacite, which has an unusually high content of the albite molecule might have to be explained by a similar mechanism. On the other hand, favouring the concept of wholesale remelting of Mesozoic quartz diorites is the very close similarity in compositions between the old plutonic rocks and the dacites, and the widespread occurrence of coarse xenocrysts of quartz and feldspar. The intermediate lavas do not have the high Fe:Mg ratios so striking in fractionation products of some basaltic magmas, notably the Skaergaard intrusion (Tilley, 1950, p. 47-49). Instead, the suite shows the characters of the orogenic volcanic rocks (Tilley, 1950,

p. 49-59): a slight but continuous increase in the ratio $\frac{FeO + Fe_2O_3}{MgO}$ toward the acid rocks, a marked scatter of points in the variation diagram at the basaltic end, and a relatively high Al_2O_3 content in the basaltics themselves. A worthy suggestion is that of Tilley's that the orogenic volcanic series originated by fractionation of a basaltic magma following contamination by sialic material.

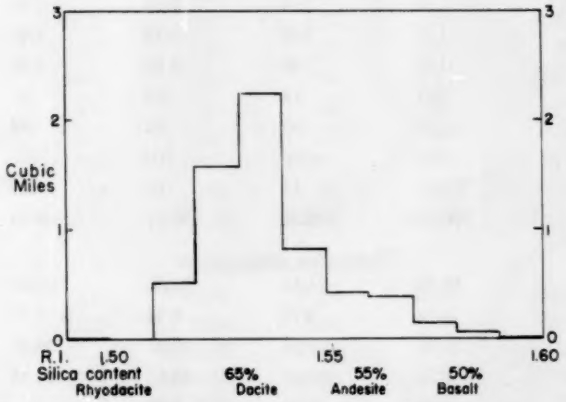

Fig. 9. Frequency diagram of acid to basic rocks of the Quaternary volcanics.

The distribution curve (fig. 9) showing the volumes of volcanic rocks of varying degrees of acidity in the map-area is not one of those which Richardson (1923) has derived on theoretical considerations for a suite formed by differentiation, syntexis, or magmatic mixing, unless a small volume of basic magma and a large volume of acid rocks or acid magma have been involved. The rocks erupted at the surface, however, probably do not provide a representative sample of the magmas developed at depth, for, other things being equal, more acid and less dense magmas would more likely be extruded. The distribution curve, skewed as it is toward the acid end, probably represents, therefore, the result of two or more processes:

(1) differentiation of a basic magma modified by syntexis,

and

(2) selection during extrusion.

TABLE 2

Chemical analyses of Quaternary Volcanic rocks of the Mount Garibaldi Map-area

	1	2	3	4	5
SiO_2	74.84	65.24	64.40	63.94	63.08
Al_2O_3	14.66	17.92	17.72	16.48	18.06
Fe_2O_3	.37	1.28	1.61	3.38	1.62
FeO	1.21	2.57	2.99	1.29	3.06
TiO_2	.12	.40	.60	.46	.58
MnO	.07	Trace	Trace	.23	Trace
CaO	1.36	4.92	5.04	5.38	5.63
MgO	.34	1.94	2.15	2.53	2.59
K_2O	2.38	1.65	1.48	1.42	1.22
Na_2O	4.49	3.46	3.26	3.64	3.72
H_2O, −105°C	Nil	.14	.12	.16	Nil
H_2O, +105°C	.25	.63	.24	.98	.08
CO_2	Nil	Nil	Nil	Nil	Nil
P_2O_5	Trace	.11	.16	.09	.07
	100.09	100.26	99.77	99.98	99.71
		Normative composition			
Quartz	35.38	24.33	24.80	22.67	19.55
Orthoclase	14.06	9.75	8.74	8.39	7.21
Albite	37.97	29.26	27.57	30.78	31.46
Anorthite	6.75	23.69	23.96	24.44	27.48
Corundum	2.22	1.76	1.98	—	.55
Diopside	—	—	—	1.29	—
Hypersthene	2.70	7.83	8.52	5.70	9.77
Olivine	—	—	—	—	—
Magnetite	.54	1.86	2.33	3.58	2.35
Hematite	—	—	—	.91	—
Ilmenite	.23	.76	1.14	.87	1.10
Apatite	Trace	.25	.37	.21	.16
H_2O+	.25	.63	.24	.98	.08
H_2O-	—	.14	.12	.16	—
Total	100.09	100.26	99.77	99.98	99.71
%An in plag.	14.3	43.4	45.0	42.8	45.0
%MgO in mafics	37.5	67.9	68.8	100.0	71.8
R. I. of fusion	1.4925	1.5295	1.5275	1.5315	1.5345

1. Devitrified rhyodacite, eastern base of Lava Peak
2. Dacite breccia, south face of Columnar Peak
3. Dacite flow, southwest end of Brohm Ridge
4. Dacite debris from glowing avalanche, Diamond Head
5. Dacite flow, Ring Creek lava, from Mamquam River near the terminus of the flow.

Analyses by W. H. Herdsman, Glasgow, Scotland.

TABLE 2 (Continued)

Chemical analyses of Quaternary Volcanic rocks of the Mount Garibaldi Map-area

	6	7	8	9	10	11
SiO_2	61.07	60.92	60.53	59.65	57.94	57.48
Al_2O_3	18.46	19.74	17.42	19.14	19.07	18.97
Fe_2O_3	1.90	2.32	2.82	1.51	2.58	2.31
FeO	3.63	2.98	2.73	3.97	3.64	3.88
TiO_2	.56	.66	1.04	.76	.82	.90
MnO	.14	.18	.12	.14	.06	.16
CaO	5.78	5.14	5.60	6.12	6.83	6.62
MgO	3.22	2.13	2.74	3.05	3.66	3.18
K_2O	.99	1.20	1.13	1.24	.78	1.04
Na_2O	4.23	3.63	3.98	3.77	3.55	4.12
H_2O, −105°C	Nil	.24	.48	.03	.14	.12
H_2O, +105°C	.07	.83	.92	.56	.66	.76
CO_2	Nil	Nil	Nil	Nil	Nil	Nil
P_2O_5	.16	.22	.21	.09	.22	.28
	100.21	100.19	99.72	100.03	99.95	99.82
			Normative composition			
Quartz	13.95	20.39	17.06	13.30	13.35	9.76
Orthoclase	5.85	7.09	6.68	7.32	4.61	6.14
Albite	35.77	30.70	33.65	31.88	30.02	34.84
Anorthite	27.62	24.06	26.33	29.78	32.44	30.21
Corundum	.31	3.66	—	.68	.50	—
Diopside	—	—	.06	—	—	.65
Hypersthene	12.45	8.10	7.99	12.64	12.42	11.63
Olivine	—	—	—	—	—	—
Magnetite	2.76	3.36	4.09	2.19	3.74	3.35
Hematite	—	—	—	—	—	—
Ilmenite	1.06	1.25	1.97	1.44	1.56	1.71
Apatite	.37	.51	.49	.21	.51	.65
H_2O+	.07	.83	.92	.56	.66	.76
H_2O-	—	.24	.48	.03	.14	.12
Total	100.21	100.19	99.72	100.03	99.95	99.82
%An in plag.	42.2	42.5	42.5	46.7	50.5	45.0
%MgO in mafics	100.0	71.3	88.3	66.3	78.3	72.1
R. I. of fusion	1.5375	1.5340	1.5365	1.5435	1.5520	1.5465

6. Dacite flow from Clinker Mountain, at The Barrier
7. Dacite dome (?) 0.8 miles northwest of Garibaldi Station
8. Dacite spine (?), The Castle, west of Squamish
9. Dacite flow, near summit of The Black Tusk
10. Dacite dike in breccia, altitude 2,800 feet, southwest slope of Round Mountain
11. Andesitic dacite, The Table.

Analyses by W. H. Herdsman, Glasgow, Scotland.

TABLE 2 (Continued)
Chemical analyses of Quaternary Volcanic rocks of the Mount Garibaldi Map-area

	12	13	14	15	16
SiO_2	55.97	54.62	52.16	49.68	48.92
Al_2O_3	16.52	19.27	16.92	16.39	18.33
Fe_2O_3	1.60	1.13	7.07	1.83	2.10
FeO	5.18	7.16	1.12	9.77	9.09
TiO_2	1.02	1.12	.88	1.34	1.88
MnO	.14	.09	.24	.26	.25
CaO	8.06	6.96	9.78	8.84	8.92
MgO	5.82	4.93	7.24	7.62	5.87
K_2O	1.24	.64	.73	.54	.64
Na_2O	3.25	3.88	3.36	2.72	3.42
H_2O, −105°C	.14	.18	.12	.38	.13
H_2O, +105°C	.82	.10	.18	.52	.33
CO_2	Nil	Nil	Nil	Nil	Nil
P_2O_5	.16	.14	.28	.23	.18
	99.92	100.22	100.08	100.12	100.06
	Normative composition				
Quartz	6.39	3.04	2.71	—	—
Orthoclase	7.32	3.78	4.31	3.19	3.78
Albite	27.48	32.81	28.41	23.00	28.92
Anorthite	26.84	33.28	28.94	30.92	32.78
Corundum	—	—	—	—	—
Diopside	9.77	.27	13.82	9.38	8.46
Hypersthene	16.53	22.66	11.62	21.38	7.24
Olivine	—	—	—	5.63	11.39
Magnetite	2.32	1.65	1.85	2.65	3.04
Hematite	—	—	5.80	—	—
Ilmenite	1.94	2.13	1.67	2.54	3.57
Apatite	.37	.32	.65	.38	.42
H_2O+	.82	.10	.18	.52	.33
H_2O-	.14	.18	.12	.38	.13
Total	99.92	100.22	100.08	100.12	100.06
%An in plag.	45.5	48.8	49.0	55.9	51.5
%MgO in mafics	73.8	60.5	100.0	62.9	61.0
R. I. of fusion	1.5640	1.5625	1.5775	1.5835	1.5845

12. Andesite, Eenostuck flow, Skookum Creek
13. Andesite bomb, The Cinder Cone
14. Oxidized 'basalt', altitude 2,600 feet, west slope of Round Mountain (buried valley of Mashiter Creek)
15. Basalt flow, west of Daisy Lake
16. Basalt flow, ½ mile northwest of The Cinder Cone.

Analyses by W. H. Herdsman, Glasgow, Scotland.

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CHEMICAL EQUILIBRIUM IN MAGMATIC GASES

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ABSTRACT. From standard thermodynamic data the theoretical compositions of some water, sulphur, carbon dioxide, gas systems are calculated for a wide range of temperatures and pressures. The systems examined are H_2O—S_2, H_2O—S_2—H_2, H_2O—S_2—O_2, and H_2O—CO_2—S_2—H_2, with the elements present roughly in the proportions found in magmatic gases. For the last named system, the variations in the theoretical molecular gas compositions with temperature are found to correspond to those in natural gases, ranging from high temperature gases with SO_2, CO and H_2, to low temperature hot spring gases which are mainly CO_2 and H_2S.

It is emphasized that a high pressure and temperature water solution of CO_2 and H_2S existing at depth in equilibrium with carbonates and sulphides in the parent lava, could, on release of pressure, give rise to the common constituents found in magmatic gases at the surface.

The fact that the presence of other constituents such as HCl, NH_3, CH_4 and COS in natural magmatic steam can also be predicted for any temperature from theory, is used as further evidence that magmatic gases closely approach a state of chemical equilibrium. The ideas of Gautier, proposed some fifty years ago, are thus confirmed by modern chemical thermodynamics.

INTRODUCTION

There have been many discussions of the chemistry of magmatic gases, often with a view to exploiting the heat from gas reactions as energy sources in volcanism.

Numerous analyses of volcanic gases exist, but there are few that satisfy the two conditions of air-free collection of the complete gas discharge, and detailed analysis by satisfactory methods. These conditions are by no means easy to achieve, and the results of most of the early collections were almost useless because of the poor techniques used.

Clarke (1916) reviewed the work of Gautier, who between 1900 and 1910 studied the products and mechanism of degassing rocks at high temperatures. The products he obtained were ascribed to reactions occurring within the rocks at the temperature. He supposed hydrogen to form from the reaction of water with ferrous silicates, and carbon monoxide from the reduction of carbon dioxide by hydrogen. He showed that sulphides were hydrolyzed by high temperature steam to hydrogen sulphide and the oxide. Further, at still higher temperatures the hydrogen sulphide formed reacted with more water to give sulphur dioxide and hydrogen. He demonstrated how a large number of reactions were possible starting with only water, carbon dioxide, and the solid constituents of lavas. Chamberlin (1908) also made a comprehensive survey of the products from degassing rocks of all types. His results were very similar to those obtained by Gautier.

Jaggar, Day and Shepherd, have all made important contributions to the literature on magmatic gases. Their excellent collections and analyses of Hawaiian volcano gases have not been bettered, and remain as a standard of comparison for any ideas on the chemistry of these gases. Shepherd (1938) also contributed many results on the degassing of igneous rocks in vacuo. On degassing Hawaiian basalts he obtained products almost identical with those collected at the same temperature over the molten lavas at Kiluaea and Mauna Loa. In this paper Shepherd warned against the strict application of thermo-

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dynamics in interpreting further the results from analyses of such gases. He considered that the combination of catalysts, catalyst poisons and adsorption effects in the rock walls would influence the attainment of equilibrium in a lava and gas system.

Even if this must be kept in mind, the early success of Gautier in producing gas compositions similar to volcanic gases from simple water, carbonate, sulphide systems, should be remembered. More recently Wilson (1939) made calculations on the thermodynamic possibility of certain compounds such as sulphur dioxide, methane, carbonyl sulphide, and carbon bisulphide being present in fumaroles and hot springs of various temperatures. He showed that the lack of sulphur dioxide and carbonyl sulphide in hot spring gases was in agreement with theoretical expectations.

Jaggar (1940) has put forward the idea of a simple gas mixture, which when dissolved in the rock, would at high temperatures give rise to the typical volcanic gas products on release of pressure. With this ideal in view it was considered of interest to calculate the theoretical combination of elements in some systems with the approximate bulk composition of natural volcanic, fumarole, and hot spring gases. The theoretical assemblies of molecules could then be compared with those found in sampling the natural gases. This would give an indication of how close the natural gases come toward attainment of equilibrium.

As the gases are observed at atmospheric pressure, any calculations should first be made for this pressure. However it would be of interest to recalculate the equilibrium conditions for gas mixtures at higher pressures, of the order of thousands of atmospheres, to get an idea of the molecular form of the gases in the original deep magma if chemical equilibrium should be preserved. If a gas system could be found that had both a logical high pressure molecular form and gave equilibrium products at various temperatures that approximated those found in nature, then this would be the ideal mixture of gases sought by Jaggar.

In magmatic gases the element sulphur shows the greatest variety of compounds. Under various conditions, sulphur vapour, sulphur dioxide, sulphur trioxide, hydrogen sulphide, and carbonyl sulphide have been found by analysis. Any theoretical examination of magmatic gases would need to be centered about the chemistry of sulphur and water mixtures at high temperatures.

Using modern thermodynamic data various gas systems are now considered. In all the calculations given, the gases are treated as ideal. Pressures are used uncorrected by fugacity coefficients. Fortunately when high pressures are involved, the temperatures of interest are usually also high, so fugacity coefficients will remain of the order of unity. As this paper deals only with broad generalizations, the numerical values given are not really important, except as a basis for comparison and discussion.

A. SYSTEM H_2O—S_2 MOLECULAR RATIO 100:1

The equilibrium constitution of an original mixture of water and sulphur molecules in the ratio of 100:1 is examined at various temperatures and pres-

sures. The ratio is of the order commonly found in natural gases. As possible final constituents S_2, H_2O, H_2S, SO_2 and H_2 are considered. Sulphur trioxide is not likely to be present. At the partial pressures of sulphur vapour involved in the following systems the only sulphur molecule of importance is diatomic sulphur S_2. (Stull, 1949).

For the calculation, the three components of the system may be chosen as S_2, SO_2 and H_2O. Equilibrium relations can then be applied to relate the other two constituents to these components.

$$K_1 = \frac{\mathbf{p}_{H_2}^4 \cdot \mathbf{p}_{SO_2}^2}{\mathbf{p}_{H_2O}^4 \cdot \mathbf{p}_{S_2}} = \frac{\mathbf{x}_{H_2}^4 \cdot \mathbf{x}_{SO_2}^2 \cdot \mathbf{P}}{\mathbf{x}_{H_2O}^4 \cdot \mathbf{x}_{S_2}} \quad (1)$$

$$K_2 = \frac{\mathbf{p}_{H_2S}^4 \cdot \mathbf{p}_{SO_2}^2}{\mathbf{p}_{H_2O}^4 \cdot \mathbf{p}_{S_2}^3} = \frac{\mathbf{x}_{H_2S}^4 \cdot \mathbf{x}_{SO_2}^2}{\mathbf{x}_{H_2O}^4 \cdot \mathbf{x}_{S_2}^3 \cdot \mathbf{P}} \quad (2)$$

These express the equilibria

$$S_2 + 4H_2O = 4H_2 + 2SO_2$$
$$3S_2 + 4H_2O = 4H_2S + 2SO_2.$$

x is the mole fraction, and **p** the partial pressure of the constituent in the final mixture.

P is the total pressure on the system. Thus $\mathbf{p}_A = \mathbf{P} \cdot \mathbf{x}_A$.

Three other relationships are needed to give five simultaneous equations that can be solved for the five constituents at any temperature and pressure. Conservation of mass equations can be used, and also the fact that the sum of mole fractions is unity. Ratio of oxygen to sulphur atoms = 50 =

$$\frac{\mathbf{x}_{H_2O} + 2\mathbf{x}_{SO_2}}{\mathbf{x}_{H_2S} + \mathbf{x}_{SO_2} + 2\mathbf{x}_{S_2}} \quad (3)$$

Ratio of hydrogen to oxygen atoms = 2 =

$$\frac{2(\mathbf{x}_{H_2O} + \mathbf{x}_{H_2S} + \mathbf{x}_{H_2})}{\mathbf{x}_{H_2O} + 2\mathbf{x}_{SO_2}} \quad (4)$$

$$\mathbf{x}_{H_2O} + \mathbf{x}_{H_2} + \mathbf{x}_{SO_2} + \mathbf{x}_{H_2S} + \mathbf{x}_{S_2} = 1 \quad (5)$$

Values of K_1 and K_2 were computed from standard thermodynamic properties of the gases. Values of standard free energies and heats of formation were taken from Latimer (1952) with the exception of the free energy of formation of diatomic sulphur gas which was taken from Kelley (1937). Heat capacity data for the gases was taken from Kelley (1949) in all cases. The calculated values of K_1 and K_2 are given in table 1.

TABLE 1

T°K	K_1	K_2
600	4.10×10^{-19}	3850
800	2.14×10^{-13}	61.4
1000	6.95×10^{-10}	5.28
1200	1.72×10^{-7}	1.09
1400	9.54×10^{-6}	0.374

Some solutions of the five equations above are given in table 2. Results are given in mole fractions $\times 10^3$.

TABLE 2

Mole Fractions $\times 10^3$

T°K	P(atmos.)	x_{H_2O}	x_{H_2S}	x_{SO_2}	x_{H_2}	x_{S_2}
600	1	980.2	13.2	6.6	0.016	0.007
800	1	979.6	13.0	6.8	0.60	0.029
1000	1	975.3	11.5	8.2	4.9	0.063
1200	1	963.5	7.5	12.1	16.8	0.079
1400	1	949.0	2.6	17.0	31.4	0.035
1200	10	970.8	10.1	9.7	9.3	0.047
1200	100	976.2	11.1	7.9	4.8	0.021
1200	1000	979.0	11.6	7.0	2.4	0.010

It is not necessary to give more than three examples at pressures other than atmospheric. The trend with increasing pressures is similar at all temperatures i.e. that of higher hydrogen sulphide and water, and lower sulphur dioxide, hydrogen and sulphur.

B. SYSTEM H_2O—H_2—S_2 MOLECULAR RATIO 100:2:1

The effect of adding hydrogen in equivalent amount to the sulphur in System A. will now be examined. Again there are five constituents as variables, and the equilibrium equations (1) and (2) can be applied. The mass balance equation (4) must be modified, but equations (3) and (5) are used again.

$$\text{Ratio of hydrogen to oxygen atoms} = \frac{204}{100} = \frac{2(x_{H_2O} + x_{H_2S} + x_{H_2})}{x_{H_2O} + 2x_{SO_2}} \qquad (4a)$$

Some solutions to the above equations for the new conditions are given in table 3.

TABLE 3

Mole Fractions $\times 10^3$

T°K	P(atmos.)	x_{H_2O}	x_{H_2S}	x_{SO_2}	x_{H_2}	x_{S_2}
600	1	980.3	19.6	0.04	0.099	0.000
800	1	978.8	19.1	0.52	1.6	0.009
1000	1	972.4	16.8	2.7	8.1	0.049
1200	1	957.9	11.7	7.5	22.8	0.105
1400	1	938.8	5.3	13.9	41.9	0.080
1200	10	966.8	15.0	4.5	13.7	0.048
1200	100	973.5	17.2	2.3	6.9	0.008
1200	1000	975.8	18.1	1.5	4.6	0.006

The effect of the extra hydrogen is to prevent the formation of sulphur dioxide at lower temperatures. The system at one atmosphere approaches a binary mixture of water and hydrogen sulphide at low temperatures, and a ternary mixture of water, sulphur dioxide, and hydrogen at high temperatures. The effect of high pressures is to favor the system with the fewest molecules, and hydrogen sulphide increases in concentration as in the first system.

C. SYSTEM H_2O—S_2—O_2 MOLECULAR RATIO 100:1:2

The constituents H_2O, SO_2, SO_3, H_2S, O_2, H_2, are considered as possible products from this system. The key reactions are

$$S_2 + 2O_2 = 2SO_2$$
$$2SO_2 + O_2 = 2SO_3$$
$$SO_2 + H_2O = SO_3 + H_2$$
$$2SO_2 + 2H_2O = 2H_2S + 3O_2$$

This system may be summarized without giving the evidence of detailed calculations. Sulphur dioxide will be formed completely before any SO_3 is formed. Sulphur dioxide will not react with water to give hydrogen sulphide and oxygen at any temperature in the range of interest, nor will water be decomposed by sulphur dioxide to give the trioxide and hydrogen. Over the whole temperature range of geological interest the same mixture of sulphur dioxide and water should be stable. Addition of more oxygen to the system would form sulphur trioxide but no other products.

D. SYSTEM H_2O—CO_2—H_2—S_2 MOLECULAR RATIO 100:10:2:1

By the addition of carbon dioxide to System B, the bulk composition of the gas approaches that often found in magmatic gases. Carbon dioxide is often present in natural steam at about 10 times the concentration of the sulphur compounds. With the addition of carbon compounds it is necessary to choose an extra component to define the system. The components may now be chosen as H_2O, CO_2, S_2, and SO_2. The constituents considered in the following calculations are H_2O, H_2S, SO_2, H_2, S_2, CO_2, CO, and COS. Four equilibrium constant equations are required to relate the constituents H_2, H_2S, CO, and COS and the chosen components. As before, equations (1) and (2) can be used, a third is equation (6) for the water gas reaction, and the fourth that for COS formation from H_2S, and CO_2 (equation (7)).

$$K_3 = \frac{x_{CO} \cdot x_{H_2O}}{x_{CO_2} \cdot x_{H_2}} \qquad (6)$$

$$K_4 = \frac{x_{COS} \cdot x_{H_2O}}{x_{CO_2} \cdot x_{H_2S}} \qquad (7)$$

Values K_3 and K_4 for various temperatures are given in Table 4.

TABLE 4

T°K	K_3	K_4
600	0.0366	7.96×10^{-3}
800	0.249	4.70×10^{-2}
1000	0.733	0.137
1200	1.45	0.278
1400	2.31	0.460

Four other equations are now required to be able to solve for the eight variables.

The O_2/H_2 atomic ratio gives $$\frac{120}{204} = \frac{x_{CO} + 2x_{CO_2} + x_{H_2S} + 2x_{SO_2} + x_{COS}}{2(x_{H_2O} + x_{H_2} + x_{H_2S})} \qquad (8)$$

The S_2/H_2 atomic ratio gives $$\frac{2}{204} = \frac{2x_{s_2} + x_{h_2s} + x_{so_2} + x_{cos}}{2(x_{h_2o} + x_{h_2} + x_{h_2s})} \quad (9)$$

The C/H_2 atomic ratio gives $$\frac{10}{204} = \frac{x_{co_2} + x_{co} + x_{cos}}{2(x_{h_2o} + x_{h_2} + x_{h_2s})} \quad (10)$$

Also $$x_{h_2o} + x_{h_2s} + x_{so_2} + x_{h_2} + x_{s_2} + x_{co_2} + x_{co} + x_{cos} = 1 \quad (11)$$

The eight equations have been solved for temperatures from 600 to 1400°K at one atmosphere pressure. In outline, the method used was successive approximation. A value for the mole fraction of water can usually be guessed with reasonable accuracy and used as a starting point. Only seven equations are then left to solve. Preliminary calculations based on System B indicate that the concentrations of S_2 and COS are relatively low compared with the other constituents. If S_2 and COS are neglected in equation (9), where the hydrogen term is small and can be given an approximate value, various ratios of x_{so_2} to x_{h_2s} may be taken to satisfy the equation. For each of these ratios, values of x_{co} and x_{co_2} are easily found by trial and error to agree with equations (6) and (10). (The values of x_{h_2} corresponding to the ratios taken are calculated by means of equations (1) and (2)). The various trial results are then tested in equation (8). If an agreement is not obtained between the two sides of this equation, the numerical difference is recorded. The process is continued with various ratios of x_{h_2s} to x_{so_2}, and a graph constructed showing the change in the numerical difference from equation (8) with changing ratio. The ratio for exact agreement can be picked from the graph. By gradually improving on the initial assumptions, results are finally obtained for all eight constituents that agree with the conditions given.

The results for the system are shown in figures 1 and 2. Figure 2 is constructed so that the difference between any two adjacent lines at constant temperature corresponds to the mole fraction of the named constituent. Results are not given for higher pressures, but the trend will be the same as in System

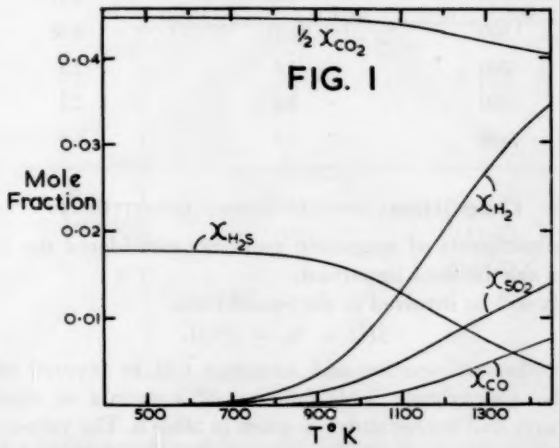

Fig. 1. Equilibrium molecular concentrations for the system H_2O—CO_2—H_2—S_2 in molecular ratio 100:10:2:1. Pressure = 1 atmosphere.

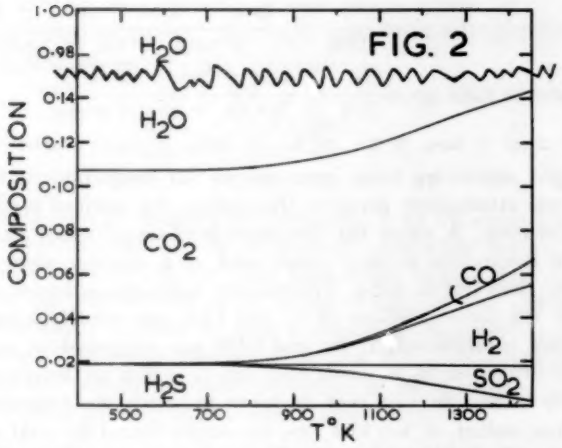

Fig. 2. Total molecular composition of the system H_2O—CO_2—H_2—S_2 in molecular ratio 100:10:2:1. Pressure = 1 atmosphere.

B. Increasing the pressure will decrease SO_2, H_2, CO and S_2, while H_2O, H_2S, CO_2, and COS will increase. Values of x_{S_2} and x_{COS} are not appreciable on the scale of the graphs, and table 5 gives their approximate values for various temperatures at one atmosphere pressure.

TABLE 5

Mole Fractions

T°K	$x_{S_2} \times 10^5$	$x_{COS} \times 10^4$
600	0.05	0.14
800	0.91	0.81
1000	4.8	2.1
1200	9.6	2.7
1400	5.3	1.5

E. EQUILIBRIA FOR ADDITIONAL CONSTITUENTS

Of the constituents of magmatic gases not considered the nitrogen and chlorine gases are the most important.

Ammonia will be involved in the equilibrium.

$$3H_2 + N_2 = 2NH_3$$

This is an exothermic reaction and ammonia will be favored at lower temperatures. The approximate mole fraction of ammonia at equilibrium for various pressures and temperatures is given in table 6. The values x_{NH_3} are for a gas containing nitrogen of constant mole fraction 0.05 and hydrogen of constant mole fraction 0.01.

TABLE 6

T°K.	P.(atmos.)	x_{NH_3}
400	1	1.3×10^{-3}
600	1	9.3×10^{-6}
800	1	6.6×10^{-7}
1200	1	4.1×10^{-8}
600	1000	9.3×10^{-3}
800	1000	6.6×10^{-4}
1200	1000	4.1×10^{-5}

At one atmosphere pressure ammonia should exist in the gases only below temperatures of about 400°C. Increased pressure can cause ammonia to become a stable constituent at higher temperatures, and it would possibly be present in the original magma at depth. That is, assuming that nitrogen is a primary magmatic gas; a fact that seems likely but has never been satisfactorily proved. The values of the equilibrium constants for the above reaction were taken from International Critical Tables (1930).

Hydrogen chloride is the most important chlorine compound found in magmatic gases. Its presence or absence in the steam will be decided by reactions involving metal chlorides, of type

$$H_2O + 2NaCl + SiO_2 = Na_2SiO_3 + 2HCl$$

High temperatures favor the right hand side of these reactions. The partial pressure of hydrogen chloride at equilibrium with a pressure of water vapor equal to one atmosphere is given in figure 3, together with the result for the equivalent calcium system. These results were calculated from the standard thermodynamic properties of the substances involved; in the case of the calcium system by Briner and Gagnaux (1948). The presence of hydrogen chloride in a magmatic gas will be dependent not only on the temperature, but on the type of chloride system present. Iron chlorides, calcium chloride,

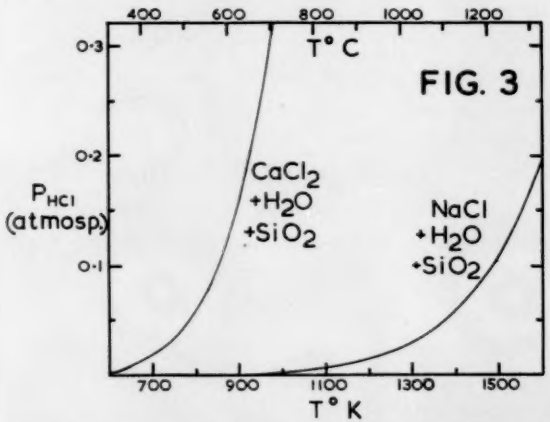

Fig. 3. Partial pressures of hydrogen chloride for the equilibria $2NaCl + SiO_2 + H_2O \rightleftarrows 2HCl + Na_2SiO_3$, and $CaCl_2 + SiO_2 + H_2O \rightleftarrows 2HCl + CaSiO_3$ at constant partial pressure of water vapour, $P_{H_2O} = 1$ atmosphere.

sodium chloride, require increasingly higher temperatures for hydrolysis to hydrogen chloride. Thus this gas may be found at relatively low temperatures of 400°C and under in an andesite fumarole area such as White Island, N. Z., but could not be expected at such low temperatures in a soda-potash rhyolitic area. Hydrogen fluoride is found in high temperature magmatic steam and results from a similar hydrolysis mechanism.

Dissociation of hydrogen chloride to chlorine and hydrogen need not be considered. Hydrogen chloride is dissociated to the extent of only 0.27 percent at 1800°K (International Critical Tables, 1930). As an approximation hydrogen chloride and nitrogen can be treated as inactive gases diluting those considered in System D, and having only a secondary effect on the equilibria.

If oxygen should be added to the system at any stage the equilibrium

$$2HCl + \tfrac{1}{2}O_2 = Cl_2 + H_2O.$$

would have to be considered. The chlorine in the Hawaiian volcano gas analyses has already been attributed by Jaggar (1940) to aerial oxidation. It may be noted also that adding a low proportion of oxygen to a system containing hydrogen sulphide could oxidise this compound only as far as sulphur and not to sulphur dioxide. This reaction is favored at low to medium temperatures.

$$H_2S + O_2 = H_2O + S_2$$

This could be the source of much of the elemental sulphur found in fumarole fields. Often in the porous ground common to such areas the fumaroles act as blowpipes, drawing in air at their bases and expelling the oxidised products in their discharges. It is a common liquid phase oxidation reaction in hot springs, producing crystalline sulphur.

Methane commonly occurs in magmatic steam. As Gautier showed as long ago as 1906 this product can be formed by the reaction of carbon monoxide with hydrogen.

$$CO + 3H_2 = CH_4 + H_2O \qquad (a)$$

The equilibrium concentration of methane can be calculated from the standard properties of the gases. For the purposes of calculation it was more convenient to consider the equation

$$CO_2 + 4H_2 = CH_4 + 2H_2O \qquad (b)$$

As carbon monoxide is formed by reduction of carbon dioxide with hydrogen, this equation for the direct formation of methane from carbon dioxide can be used. For a gas of original composition

$$x_{H_2O} = 0.89,\ x_{CO_2} = 0.10,\ x_{H_2} = 0.01,$$

the results at equilibrium at one atmosphere pressure for x_{CH_4} and x_{H_2} are given in table 7. Values of the equilibrium constant for the equation (b) are tabulated for various temperatures.

$$K_P = \frac{p_{CH_4} \cdot p_{H_2O}^2}{p_{H_2}^4 \cdot p_{CO_2}}$$

TABLE 7

T°K	K_P	$x_{H_2} \times 10^2$	x_{CH_4}
1000	2.78×10^{-2}	1.00	negligible
700	4.21×10^{2}	1.00	negligible
600	7.40×10^{4}	0.97	7.4×10^{-5}
500	8.73×10^{7}	0.36	1.6×10^{-3}
400	2.75×10^{12}	0.03	2.4×10^{-2}

SOME ANALYTICAL RESULTS

Some results from analyses of magmatic gases may be reviewed. There are few reasonably complete analyses available. Probably the best collections and analyses of high temperature volcanic gases are the series of Shepherd and Jaggar taken from molten basalt at Hawaii in the years 1917-19. Jaggar (1940) has reveiwed these analyses and graded them according to the conditions of collection of the sample.

Six of the samples, with highest hydrogen content, and graded excellent by Jaggar may be taken as typical of the samples with least contamination.

Fumaroles in the andesite cone of White Island in the Bay of Plenty, off the North Island of New Zealand, provide a good example of the medium temperature type of gas discharge. The hottest fumaroles have temperatures of the order of 600°C. Wilson (1953) has published analytical data for this source.

White Island

Condensate from Fumaroles (Parts by Weight)

H_2O (100), Total sulphur as H_2S(0.16) HCl(0.91), HF(0.01), H_3BO_3(0.01), NH_3(0.002). A weighted average from 4 fumarole areas.

Analysis of Uncondensed Gas (% by volume)

H_2S(2.0), CO_2(86.3), H_2(9.8), N_2(1.9), CH_4(0.00).

Unfortunately these results do not emphasize an important characteristic of this fumarole area. The total sulphur compounds in the condensate were calculated as hydrogen sulphide, but to anyone visiting the island it is obvious that both hydrogen sulphide and sulphur dioxide are being expelled in the fumarole gases. It is interesting to note that the hydrogen chloride content of the steam is of the order predicted from theory for the system $CaCl_2$—H_2O—SiO_2 at the expected partial pressure of water vapor of about 10 atmospheres.

Wilson (1941, 1953) has previously shown how the presence of polythionates in hot springs can be used as an indication of the presence of both hydrogen sulphide and sulphur dioxide in the magmatic steam heating the water. He gave examples where the presence of sulphur dioxide shown in this manner could be correlated with the higher temperature conditions of the more recently active volcanic areas in New Zealand.

The presence of both sulphur dioxide and hydrogen sulphide was shown by analyses of gas samples from Lassen Peak fumaroles during years of active volcanic activity. This was reported by Day and Allen (1925). The tempera-

TABLE 8

Sample No.	CO_2	CO	H_2	N_2	A	SO_2	S_2	SO_3	Cl_2	H_2O	Total
S7	17.25	0.62	0.76	5.88	0.18	9.75	1.07	0.00	0.25	64.18	99.94
J18	17.55	0.74	0.83	4.50	0.12	10.81	0.22	3.22	0.13	61.88	100.00
J13	16.96	0.58	0.96	3.35	0.66	7.91	0.09	2.46	0.10	67.52	100.59
S2	17.95	0.36	1.35	37.84	Udt	3.51	0.49	—	Udt	38.48	99.98
S5	9.54	1.12	1.53	10.47	0.00	9.90	2.72	—	0.00	64.71	99.99
S_3	33.48	1.42	1.56	12.88	0.45	29.83	1.79	—	0.17	17.97	99.55

Volume percentages at 1200°C, 760mm pressure.

tures of the fumaroles were not reported, but from the description it seems that the conditions were very similar to those in the present White Island fumarole area. Their results are reproduced in table 9.

TABLE 9

Gases soluble in $Ba(OH)_2$. Percent by volume

	CO_2	SO_2	H_2S	HCl	HF
Fumarole, Lassen Peak, east side of crater of 1914; collected June 1916.	0.096	0.054	0.005	none	none
Fumarole, south side of old crater; collected by E. S. Shepherd, June 1915.	0.040	0.005	0.004	—	—

Clark (1916) summarized many examples of early analyses of gases from Vulcano and Vesuvius. The results show the change from hydrogen chloride, sulphur dioxide gases of high temperature sources to hydrogen sulphide, carbon dioxide compositions at lower temperatures.

Many analyses exist for low temperature gases from hydrothermal areas such as Yellowstone National Park (Allen and Day, 1935); Iceland (Barth, 1950); The Geysers, California (Allen and Day, 1927); and Wairakei, New Zealand. They are of similar general type, and a few examples are given in table 10. The bores for geothermal power at Wairakei, ranging in depth from 600 to 3000 ft. have provided the best samples of the complete hydrothermal fluid yet taken. At the bottom of these bores the temperatures are about 250°C, but the gases sampled from this deep hot water are very similar to those found in the steam from the natural fumaroles and boiling surface pools of this area. The bores discharge a mixture of steam and water in the ratio expected from the adiabatic expansion of a liquid water phase at about 250°C to lower pressures at the surface (Ellis and Wilson, 1955). The two analyses kindly supplied by Mr. S. H. Wilson, Dominion Laboratory, as illustrative examples, are for steam separated from the water/steam mixtures at 99°C and local atmospheric pressure. Karapiti fumarole discharges dry steam into the atmosphere at about 130°C.

TABLE 10

Parts per thousand, by volume at atmosphere pressure

Source	H_2O	H_2S	CO_2	H_2	CH_4	N_2+A	NH_3	Temp. °C
Bore No. 4/1, Wairakei	999.600	0.033	0.329	0.009	0.003	0.005	0.021	250
Bore No. 9 Wairakei	999.310	0.050	0.608	0.014	0.006	—	0.012	250
Karapiti Fumarole Wairakei	999.530	0.033	0.429	0.002	Trace	0.002	0.004	130
Black Growler, Norris Basin, Yellowstone.	996.00	0.10	3.86	0.02	0.01	0.02	—	130 (about)
Well No. 2, The Geysers, California.	986.86	0.42	7.77	2.16	2.08	0.48	0.23	160
Spring at Reykir in Olfus, Iceland*	990.00	0.82	8.22	0.53	0.03	0.40	—	99

* assumed 1% gases other than water.

DISCUSSION

The results from both the theoretical calculations for certain systems, and from analysis of collected magmatic gases have been presented. Any connection between the two may now be examined.

The discussion will be centered about the idea put forward by Jaggar (1940) and mentioned earlier, of a simple gas mixture which could be dissolved in the igneous rock, and could give on release of pressure the typical magmatic gas products that are found by analysis for particular temperature conditions.

If there should be such a gas mixture the predominant sulphur equilibrium within it could be one of three main types.

A. Insufficient hydrogen, other than in water molecules to reduce all the sulphur dioxide to hydrogen sulphide at low temperatures.

B. Hydrogen, other than in water, present in at least equivalent amount to sulphur.

C. Sulphur dioxide present not dependent on the decomposition of water.

The calculations given are for the extreme type of each system. The CO_2—CO—COS equilibria may be superimposed on these simpler systems without seriously altering the conclusions which can be drawn. The nitrogen and chlorine equilibria can also be considered separately as a first approximation.

Let us for the present assume that magmatic gases approach a state of chemical equilibrium. The analytical results show that sulphur dioxide gradually disappears from the gases as the temperature becomes lower. For systems of type C sulphur dioxide is the stable product at all temperatures of interest. As this type of system does not produce the other sulphur products common to magmatic gases it need not be considered further.

Systems of type A have sulphur dioxide stable at all temperatures, but in equilibrium with hydrogen sulphide at low temperatures. It is possible that the sulphur dioxide could be lost preferentially from a magmatic gas of this type as cooler conditions were approached at the surface, so that only hydrogen sulphide was expelled in the low temperature surface gases. The effect of increasing the pressure, however, is similar to lowering the temperature, and causes the composition to approach a two to one mixture of hydrogen sulphide and sulphur dioxide in the ideal case examined. This should be the original type of composition of the deep high pressure gases for this system. Sulphates or sulphites therefore could be expected in a cooled magma that has retained its gases, but they are not common constituents of plutonic rocks. It does not seem likely that a system of Type A predominates in magmatic gases.

Systems of type B and D account for the general differences found between high and low temperature magmatic gases without any further assumptions than the initial one of chemical equilibrium between constituents. The effect of pressure on these systems is to favor water, carbon dioxide, and hydrogen sulphide which are the most likely constituents in the magma from which the gases originate. The hydrogen sulphide and carbon dioxide would exist in equilibrium with sulphides, carbonates, and high temperature water fluid, all dissolved in the hot magma. Should a high pressure gas phase con-

sisting mainly of water, carbon dioxide, and hydrogen sulphide be formed, it would subsequently have the chemical flexibility to form the common constituents found in surface magmatic gases. Thus the earlier ideas of Gautier on this type of system are confirmed by the application of modern thermodynamic data.

There is no doubt of the similarity between the theoretical results given for system D and the analytical results for the same temperatures. Only the general character of the gases can be compared, as there are wide variations in the concentrations of constituents in the various collections of gases from different sources. However, the high temperature CO, H_2, SO_2, COS constituents are predicted by calculations, as are the low temperature CO_2, H_2S compositions. It is likely that the total sulphur contents of gases from volcanic craters are much higher than in the average gas liberated from a magma, because of recycling of sulphur within the craters.

Comparison of the Hawaiian analyses with the theoretical composition of system D shows that the hydrogen content found by analysis is lower than predicted. A paper by Kennedy (1948) is interesting in this respect. From the average ferric/ferrous ratio in fresh Hawaiian basalts be deduced that there should be approximately 0.16 percent hydrogen in the volcanic gases, and providing equilibrium was maintained between lava and gas phases, a greater proportion of hydrogen should not be expected. The best of the analyses reported by Jaggar however have a higher hydrogen content than this. Also, from experimental work Kennedy showed that at 1200°C at least 20 hours were required for iron oxides to come to equilibrium with the gas phase. It appears that the hydrogen content of volcanic gases is lowered by reaction with ferric iron, but the low equilibrium content is not always obtained. The output of ferric iron in lava would have to be much in excess of the gas output of the volcano for the hydrogen concentration to be controlled in this way.

Gases with a hydrogen content of the order of 10 percent have been collected by Day and Shepherd in 1912 (Jaggar, 1940) by pumping gases from flame cracks in Hawaiian basalt. It seems that the removal of hydrogen from a magmatic gas stream by ferric iron is a variable effect depending on the velocity of gas discharge, and the kinetics of the various reactions. It is undoubtedly important, but because of its variable nature is difficult to allow for in a theory as general as the present one. The presence of nitrogen in most analyses adds the possibility of aerial oxidation of hydrogen, and makes the problem still more difficult to assess.

It is of course impossible that a gas system of the exact bulk composition corresponding to an original water, carbon dioxide, hydrogen sulphide mixture will remain intact at all temperatures in all volcanic areas. Under suitable conditions there will be deposition of sulphides, or of sulphates; perhaps oxidation by rocks containing ferric iron, or by entry of air to the system. It is believed that each individual case could be treated as a variation of this original mixture with due allowance for local conditions, such as mineral formation or oxidation. By adding to the products of this basic system the concentrations of hydrogen chloride, ammonia and methane predicted in

figure 3 and tables 6 and 7, a very fair approximation of the various magmatic gases found in the field is obtained.

It is not expected that exact chemical equilibrium will always be established in magmatic gases. Indeed the opposite would be more correct. It is likely however, that in the majority of cases the systems approach the equilibrium conditions quite closely, especially within the gas phase. With extremely rapid processes such as explosive volcanic eruptions the constitution of the gases liberated would resemble more closely those predicted for deeper high pressure conditions than for atmospheric pressure. Thus hydrogen sulphide and ammonia might be expected in high temperature gases under such circumstances.

Some consequences may be considered of assuming the basic character of magmatic gases is due to equilibrium changes within a simple gas system.

It is not necessary for hydrogen to be an original component in the magma. It can be produced by the reaction of hydrogen sulphide with water at high temperatures. If a certain amount of sulphur dioxide should be lost from high temperature steam by neutralisation reactions, three times its molecular concentration of hydrogen would be left permanently in the gas to exist along with hydrogen sulphide at low temperatures.

$$H_2S + 2H_2O = SO_2 + 3H_2$$

As the concentration of hydrogen is usually low in cooler hydrothermal gases, only a small proportion of sulphur as sulphur dioxide would need to be lost in this way.

Carbon monoxide would not be a constituent at high pressures in the original magma. Hydrogen and carbon monoxide were considered by Jaggar to be fundamental exudations from the inner earth. This does not seem to be a necessary assumption in the light of the present work.

It is often considered that elemental sulphur is the original sulphur compound in the deep magma. From the systems investigated it appears that hydrogen sulphide is more likely to be that compound. Sulphur could be deposited either from the equilibrium proportion of sulphur vapor present in the gases or from partial oxidation of hydrogen sulphide.

Carbonyl sulphide would be an equilibrium constituent of medium temperature magmatic steam, reaching a maximum concentration at about 1200°K. The instability of hydrogen sulphide in high temperature steam reduces the concentration of carbonyl sulphide at temperatures above this. The concentration would never become very high.

Equilibrium relations for both methane and ammonia were calculated separately from system D. This was done partly for simplicity, but mainly because misleading results would be obtained by their direct addition to the constituents of this system. As hydrogen disappears almost entirely at low temperatures in the ideal closed system, ammonia and methane would not appear. However, free hydrogen is found in almost all magmatic gas samples not contaminated by oxygen, even at lowest temperatures. A possible mechanism for the existence of this hydrogen has been given. By taking reasonable and appreciable values for the mole fraction of hydrogen at low temperatures the possibility of ammonia and methane formation is more correctly shown.

There is no heat liberated by the change from high pressure to low pressure constituents at high temperatures. The processes in a rising volcanic gas would be adiabatic and not isothermal but values of ΔH in cals/mole for certain key reactions can be given for a temperature of 1200°K.

$$H_2S + 2H_2O = 3H_2 + SO_2. \quad \Delta H°_{1200} = 54400$$
$$CO_2 + H_2 = CO + H_2O. \quad \Delta H°_{1200} = 7900.$$

There would be no spontaneous rise in temperature because of internal chemical reaction in the gases on releasing the pressure as was visualized by Jaggar for a basic magmatic gas mixture.

In conclusion it may be said that the agreement between field results and those calculated is an indication that magmatic gases approach a state of chemical equilibrium, and that the usual thermodynamics of gas reactions can be usefully applied to such problems. It appears that Shepherd was unduly pessimistic about the effects of contamination and adsorption ruling out further interpretation of the constituents found by sampling volcanic gases.

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REMARKS ON THE GENESIS OF FLINTS

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ABSTRACT. Ideas on the origin of flints have varied widely. Flints may have been formed by several different processes. For the flints typically found in the chalk, the syn-sedimentary replacement theory, pene-contemporaneous with the deposition of the chalk, has largely replaced the older diagenetic replacement theory. The finding in flints of extremely well preserved microfossils, still formed of organic material, even pointed to an almost unbelievably swift replacement. Palynological studies, by underlining the extreme toughness of these microfossils, and chromatographic analyses of macrofossils, by showing that organic material is widely present in common fossils, has lessened this difficulty.

No flint has yet been encountered forming at the present time. This is not surprising as flint should form in an environment hardly represented at present, namely, a warm, shallow sea, without terrigenous sedimentation, with a calcareous bottom but temporarily lacking even calcareous deposition. Most localities suitable for flint formation were obviously drowned by post-glacial eustatic rise of sea level.

INTRODUCTION

Flints, composed of crypto-crystalline quartz, are common in many formations. Their form is extremely variable, as is, to all appearance, their genesis. I shall therefore restrict these remarks to the flints of a very irregular form, each flint being built up individually by the fusing of a number of separate nodules of silicification. Such flints are typically found, for instance, in definite layers in the Upper Cretaceous of Europe. These flints, the "silex de la Craie", or the "Kreidefeuersteine", are associated with the chalk facies of the Cretaceous, the "Craie tuffeuse" or "Tuffkreide".

Well-rounded flints, also found in the chalk, may or may not have the same origin as the flints of irregular form. In some cases they are seen to be derived from irregular flints by erosion and abrasion, becoming re-deposited in the chalk somewhat higher up in the series. These questions are not further considered here. Nor do these remarks cover, for instance, the flints found in the limestone series of the Lower Carboniferous of western Europe (Cayeux, 1929). Their genesis may be quite similar to the flints of the chalk, but their more regular general form may well be due to later recrystallization in the Variscan geosyncline or during the Variscan orogeny.

I wish to restrict these remarks to the flints as they were when first formed by metasomatic recrystallization of limestone, and not take into account any later influences. My concern is with those flints which one would call "primary", were it not that their origin is already of a secondary nature.

THE DIAGENETIC THEORY

That the flints of the chalk are a secondary product, formed by metasomatic recrystallization of the original limestone, is proven by the many silicified fossils found in the flint, and by the fossils only partly silicified, partly within a flint nodule and partly still in the surrounding chalk.

Although there is no doubt as to the secondary nature of the flints, different views are possible as to the time of silicification.

Originally, the silicification was classed as a diagenetic process, taking place very slowly during the time between the formation of the chalk series and the present, a process taking many millions of years. Evidence for the

circulation of vadose waters carrying a surplus of SiO_2 in the limestone environment of the chalk series is found in the deposits of crypto-crystalline quartz—resembling flint in all but its outer form and its position—on diaclases (joints) in the chalk.

There is, I think, no doubt about the origin during a much later period, of these flatwalled, thin, even sheets of "flint". These "Feuersteinvorhänge" or "flint-curtains" follow diaclases which cut through all or most of the complete series of the chalk. But convincing evidence has accumulated in later years that the normal, irregular flints of the chalk, found in separate layers in the series, were formed in another way.

CAYEUX AND PENE = CONTEMPORANEOUS FORMATION OF FLINTS

A major attack on the theory of later, diagenetic formation of flints was launched by Cayeux (1897). A great many detailed observations on the "silex de la Craie" in northern France had supplied him with numerous examples of flints having developed almost directly after the formation of the layer of chalk in which they are found. Flints, for instance, broken up into sharp-edged fragments by intraformational erosion and re-deposited in the next layer of chalk, prove that silicification was already complete, before the formation of that next layer of chalk. Consequently, Cayeux regarded the origin of the flints of the chalk as syn-sedimentary or pene-contemporaneous, and not due to a much later process.

It follows that the apparently tranquil and regular series of the chalk is formed by a succession of rather different types of environmental circumstances, rhythmically following each other. There is first the period of chalk sedimentation, characterized by an open, well-aerated sea floor, without terrigeneous sedimentary supply, resulting in the formation of a rather pure organo-detrital limestone. Then follows a period of non-deposition with local superficial silicification of the newly formed limestone, proceeding from a great number of separate silicification nuclei. Thereafter, the "normal" environment of the chalk sea may have been re-established, or a longer period of non-deposition, or even a period of erosion, may have followed, before the next advent of the chalk sea.

It is from the latter periods, which are not necessarily developed in every small-scale chalk sea rhythm, that Cayeux's observations on an early formation of flints are derived.

Cayeux's ideas on the pene-contemporaneous origin of the chalk flints were rather revolutionary. They even led him to the thesis that the formation of the chalk flints and related formations were due to nonactualistic processes; the "causes anciennes en géologie" (Cayeux, 1941). Although these conclusions were disputed by Lafitte (1949) and myself (Rutten, 1949), I wish to stress that only the non-actualistic conclusions were questioned. The factual observations of Cayeux stand as before, and have been reinforced by later work.

IRREGULARITIES IN THE CHALK SERIES

I think that everyone who has studied chalk series in detail will, in due time, have found similar indications of flints, eroded, reworked or abraded,

and redeposited in a chalk layer immediately following that from which the flints were taken. In every such case, the flints behave as tough bodies, with typical conchoidal fracture. They were already silicified, whereas no subsequent silicification has taken place in their secondary place of sedimentation.

As a matter of fact, detailed bed for bed analysis of a great number of series, not only of the chalk, but also of comparable facies, has brought to light that the "tranquility" of these series is entirely misleading. On the contrary, they are the result of several different environments, subsequently changing and following each other in a rhythmic pattern. Moreover, they are cut by larger irregularities, leading to periods of non-deposition, to hardgrounds or to actual erosion and emersion. All these features are normally included in these apparently tranquil and concordant series.

The genesis of the flints of the chalk accordingly takes its place within the series of rather variable periods of sedimentation and non-sedimentation, which made up the chalk. It is, in a sense, comparable to the dolomitization of a limestone series. There also we have learned to distinguish between a later wholesale post-sedimentary dolomitization and the pene-contemporaneous dolomitization of specific beds of limestone.

TIME OF SILICIFICATION

Geologically speaking, the local silicification of the limestone of the chalk into the flints, although secondary, consequently is an early process. As it follows immediately upon sedimentation, it is called syn-sedimentary or pene-contemporaneous.

As to the number of years this process has actually taken, we have no definite clue. The "geologically" short period between the formation of one layer of chalk and the next younger one may well be counted in thousands of years.

A series of limestone beds is not formed by a constant process. In that case there would be no beds or bedding planes, but only a massive limestone. Every bedding plane is the expression of an interruption in, or a change of, the sedimentation process. Every bedding plane forms an indication that for a given time other conditions held sway than those leading to limestone sedimentation. Every bedding plane might in fact represent a diastem, a period of non-deposition. But there are very few facts that allow an absolute, or even a relative measure for the time taken up by the formation of the bed and that of the bedding plane.

Sonder (1956) recently stressed the importance of the bedding-plane diastems in an apparently continuous series. With characteristic impetuousness, he arrives at more or less precise figures for the duration of the periods in which beds and bedding planes were formed. Unfortunately, these figures are based on estimates of speed of sedimentation which are far from exact. They are not worthy of consideration, beyond the fact that they too, indicate the possibility of thousand-year periods for the formation of single diastems.

It is of importance to realize the extremely long time, as counted in years, involved in a process like the formation of flints, which geologically is an early process. The silicification of the newly formed chalk on the sea floor, during

a period of non-deposition, or even perhaps of partial emersion, is pretty sure to be under the influence of biochemical processes. The latter are regulated by yearly or even seasonal changes. Consequently, thousand-year periods, available for the formation of bedding-plane diastems or for the formation of flints, before the sedimentation of the next higher chalk layer begins, are very long indeed from a biochemical standpoint, although they may be short-lived and pene-contemporaneous, when viewed from the angle of the stratigraphical column.

THE MICROFOSSILS

Although no further indications as to the time needed by the silicification process could be gained from the macroscopic examination of the flints, evidence for a very quick silicification was provided by some of the microfossils. These indications came from the studies of unicellular microfossils, mainly by O. Wetzel and G. Deflandre.

Wetzel (1933 and many other papers) recognized that unicellular micro-organisms were fossilized in the flints as organic substances. They were not silicified. Although these fossils are resistant against strong acids and bases, this seems to indicate almost immediate fossilization in the siliceous material.

This opinion was emphasized even more strongly by Deflandre (for instance 1936), who not only described *Hystrichosphaeridae* with beautifully preserved pseudopodia, but also *Flagellata* with flagellae of a thinness and length that is difficult to obtain by the most careful preparation of recent material.

Moreover Deflandre (1935a) succeeded in coloring the microfossils contained in flints by normal biological coloring agents, such as ruthenium red, eosine, fuchsine, methylene blue and dahlia violet. The fact that some coloring agents, such as hemalum and fast green had no effect on the microfossils, might of course be due to a degradation of the organic material during fossilization, but it can also be explained by some influence of the surrounding SiO_2.

Since it is our experience that, in general, organic material is destroyed rather quickly, say in a matter of years, these observations point to an almost immediate fossilization of the micro-organisms in the flints. From a normal, geological early process of syn-sedimentary character, the silicification became something like a spontaneous process. It seemed as if it occurred almost spontaneously, *à la minute*, following directly upon the sedimentation of the limestone in the chalk.

Newer observations have taught us, however, not to place too much emphasis on the organic nature of the microfossils in the flints. They come from two different sources, viz. from palynology and from a general chromatographic study of normal fossils.

In palynological studies of Tertiary deposits in Venezuela, Kuyl, Muller and Waterbolk (1955), for instance, reported that *Hystrichosphaeridae* occur not only in lignites but in shales as well. They belong to the most resistant of microfossils. They have not only withstood fossilization in normal sediments, but also the subsequent ordeal of the preparation of palynological samples.

The latter includes boiling with HF, HNO_3, HCl, KOH and a mixture of H_2SO_4 and acetic acid, all in rather strong concentrations.

These palynological studies confirm the fact, already stressed by O. Wetzel (1933), that these micro-organisms, although consisting of organic material, are remarkably resistant.

Modern paper chromatography, on the other hand has shown that "normal" fossils, such as pelecypod shells, often retain protein and its amino-acid degradation products over long periods in geological history (Abelson, 1956).

It follows that the coloring of microfossils contained in flints by Deflandre likewise is not conclusive as to a very sudden, *à la minute* silicification and fossilisation.

THE SILICIFICATION PROCESS

From these newer facts we may conclude that the genesis of chalk flints is a more or less normal, early geological feature. The flints are of a secondary nature, and were formed syn-sedimentarily by a silicification following the deposition of the organo-detrital limestone of the chalk.

Although we may arrive at a definite idea of the time at which the silicification took place, and while we can indicate, in a general way, what its environment was, we have no idea about the details of the subject. The theory, put forward to explain the immediate, *à la minute* silicification, still is very attractive. This surmises temporary, partial emersion, in quiet environment, leading to a period of a warm, very shallow sea, in which *Silicospongiae* flourished. Locally, a silica gel would have formed on the sea floor, which reacted with the underlying limestone of the chalk, leading to local metasomatic replacement of $CaCO_3$ by SiO_2.

Siliceous sponges, with their small needles, which fall apart when the animal dies, may be very propitious in forming a temporary overabundance of silica on the sea floor, but other organisms, micro-organisms in particular, may be responsible for this phenomenon also (Douvillé, 1932).

Newell et al. (1953) also use the silica of siliceous sponge needles to explain both the formation of siliceous nodules and the de-silification and calcification of the sponge needles. There are, however, some differences between their nodules and the flints of the chalk. The flints are related to diastems and are formed on top of an underlying layer of chalk. Moreover, as Cayeux demonstrated in many cases, they were formed before the next layer of chalk was deposited. Also, no large amounts of sponge needles are found in the chalk, or they may even be absent altogether.

The process proposed by Newell et al. operates in the uppermost limestone layers, the process proposed by Cayeux and Douvillé on the surface of a chalk layer, during a temporary exuberance of Silicospongiae. These two processes presumably are not exclusive, but may have operated each in its own area and environment. Newell et al. postulated solutions of sufficient concentration for the chemical reactions of desilicification and calcification of the sponge needles and the formation of siliceous nodules. Cayeux stressed the influence of microbes in replacement reactions taking place in warm and shallow seas.

Personally, I believe Cayeux made a very strong point in stressing biochemical activities in these replacement processes. In that way a silica gel could form in surroundings where no purely chemical silicification is possible. Microbiologists often criticize their fellow scientists as apt to forget the immense importance of microbes in all matters of life and decay, and geologists are probably the worst offenders. If our present civilization were fossilized, one would, perhaps, be able to recognize a dairy from a bakery, a brewery or a "*cave coopérative*" in wine-growing France, but never would the microbes underlying the different processes characterizing these industries be visible. The importance of microbiology for sedimentation processes in warm, shallow seas in a calcareous environment, has for instance recently been stressed by Nesteroff (1956).

Cayeux's theory is to be sure no more than an *ad hoc* hypothesis. There is no detailed knowledge of the processes leading to the genesis of flints, because, as yet, we know no part of the seven seas where flint is forming now.

This is, of course, an argument for the old theory of formation of flint through later diagenetic processes, while it is against the ideas put forward in this paper, of pene-contemporaneous silicification, geologically synchronous with sedimentation, and occurring on the sea floor during periods of altered environmental conditions.

GENESIS OF FLINTS AND ACTUALISM

"Show me a single spot on earth", the diehards of the diagenetic theory will say, "where flints are formed now, and I will be convinced". Unfortunately, we are, as yet, unable to show them such a locality.

This is a difficulty encountered not only in the genesis of flints. We meet it again, at least partly, in other processes which are of a syn-sedimentary character. Bed-for-bed silicification or dolomitization of limestones and the formation of iron oolites are comparable cases. This difficulty led Cayeux to abandon actualism (uniformitarianism) and to suppose the existence of "*causes anciennes*".

Personally, I have always held that it is not necessary to abandon actualism, if only two factors are considered which are, I think, of great importance. First, actualism does not mean that processes now active have always been as active in geological history. Also, processes, possible now, but relatively inactive, might have been far more active formerly.

In this context, it should always be borne in mind, that we live in a very peculiar period of the Earth's history, which is characterized by being both interglacial and post-orogenic. In contrast, the more normal periods of the Earth's history—and the formation of the chalk during the Upper Cretaceous is very typical indeed—are characterized by being non-glacial and a-tectonic (Rutten, 1953).

Secondly, it is a well known fact that marine geology is still in its infancy. Notwithstanding the many cruises made, there is hardly any area of the seven seas where the environment provided by the seasonal changes of the seawater, and its interrelation with fauna and flora, has been studied over more than a decade.

As to the first point, the difference between our present-day shallow seas and those, for instance, of the Upper Cretaceous must be very great indeed. All former shallow seas, where sedimentation had built the sea floor up to effective erosion through wave action, were drowned, together with the adjoining continental lowlands, through the post-glacial eustatic rise in sea level. Our "shelf seas" consequently do not form a geologic unity, nor is the "neritic zone" a biological unity. The shelf sea is composed of deeper elements of former littoral and "neritic" character and higher elements of former continental character. Biologically, the shelf seas consist of a supra-neritic zone of some 40 m to 50 m deep, in which vigorous life is possible and which forms a biological unit. To this supra-neritic zone is added a deeper zone, only because of the convenience of the 100-fathom line. The lower boundary of our present "neritic" zone is indicated by the amount of the post-glacial eustatic rise of the sea level. It has nothing to do with biology, and it does not constitute a definite boundary of an ecological zone.

Moreover, we are now in a period of strong continental erosion, following upon the uplift of many areas of our continents, which in turn is probably the direct consequence of the Alpine orogeny. Tectonically speaking, our present day circumstances are comparable to those of the Old Red and the New Red periods, with extensive continental masses, and terrigene sedimentation strongly prevalent. This is in total contrast with, for instance, the Upper Cretaceous, when—at least in Europe—tectonic unrest was at its minimum, and calcareous sedimentation, without terrigenous supply, prevailed.

We must imagine the Upper Cretaceous chalk sea as a very wide, shallow, well aerated sea, bordered by a base-levelled continent, from which no terrigenous sedimentary supply was derived. It was not a shelf sea in our present-day sense of the word, and "epicontinental sea" would perhaps be the best word for it. The climate was tropical. The shallow sea was so wide, that tidal influences were not felt, but an occasional hurricane might have been important in churning up the soft, not yet consolidated, calcareous detritus.

Small rhythmic crustal movements of cyclothem or even sub-cyclothem scale, had great influence in regulating the succession of sedimentation periods and their alternation with periods of non-sedimentation, of erosion or emersion. During the latter periods, the circumstances were repeatedly favorable for local development of a silica surplus on the sea floor, and subsequent superficial silicification of the limestone and the formation of flints.

HOW TO LOOK FOR RECENT FLINT FORMATION

Where, we may ask, will we find at present the conditions of the chalk sea during the Upper Cretaceous? The search is limited to areas with a tropical or subtropical climate; areas where recent sedimentation has offset the post-glacial eustatic rise of the sea level; and areas where there is no appreciable terrigenous sedimentary supply.

Truly, there are, from the Bahama Bank to various bays of the Red Sea, along the northern coasts of Australia, and in atolls of the Coral Sea, local areas, where calcareous sedimentation has offset the post-glacial rise of the sea level. In these scattered places we may locally study the formation of coral reefs, of rocks of the reef-complex, or of calcareous oolites.

These areas are, however, characterized by the deposits belonging to a period of vigorous calcareous organic or organo-detrital sedimentation. It will be only through a freak of the environmental conditions that we will find at present an area of non-deposition, within an environment of calcareous sedimentation, which is still so near to the surface, that silica-secreting biochemical reactions may flourish there. The correlation of the formation of the chalk flints to periods of non-sedimentation is in itself almost prohibitive to the finding of such an area at present.

All normal areas of non-deposition have been covered by 80 m of sea water during the last 10,000 years of the Holocene. So we must search for the chance of an abnormal area. This might be, for instance, a spot where crustal movements have offset the post-glacial drowning. Or where, some short while ago, calcareous sedimentation has outgrown the drowning.

The chance of finding such an abnormal area is not very great. But even if it is comparable to the needle in the haystack, a search can be conducted on better lines, if one knows what to look for. In the case of present-day flint formation, this must consequently be an area of non-deposition in an environment of a warm, shallow sea with calcareous sedimentation, without terrigenous sedimentary supply. Personally I am fully confident that the exploration teams of marine geology will find in due course such an example of present-day flint formation.

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REVIEWS

The Quaternary Era, with Special Reference to its Glaciation; by J. K. CHARLESWORTH. 2 vols., p. xlvii, 1700; 326 figs., 32 pls. London, 1956. (Edward Arnold,[1] $50.00).—This is the fourth comprehensive work on the Pleistocene to come from Britain, having been preceded by Geikie's *The Great Ice Age*, W. B. Wright's *The Quaternary Ice Age*, and Zeuner's *The Pleistocene Period*. It is the result of 35 years' labor by its author, who is Professor Emeritus of Geology in Queen's University, Belfast. Like its predecessors it covers the entire field of Pleistocene studies, and like the volumes by Geikie and Wright it lays chief emphasis on glaciation. Part 1 (about 14% of the text) is devoted to glaciology, part 2 (about 23%) to glacial geology, and part 3 (about 63%) to the glaciation, geography, stratigraphy, paleontology, and prehistoric archeology of the Pleistocene. Part 3 includes chapters on volcanic and tectonic activity, post-glacial submergence and emergence, and the reimmigration of organisms into the deglaciated regions. Part 2 includes chapters on sea ice as a geologic agent and on "cryoturbation."

This massive and learned book is essentially a compendium, one might almost say an encyclopedia. The list of references at its end includes 1924 titles arranged alphabetically by authors. At the end of each of the 51 chapters is a list of references, in abbreviated form, which serve also as bibliographic footnotes only partly represented in the main reference list. For most chapters these lists run into the hundreds of entries. In one chapter (Pleistocene climatology) with 40 pages of text, there are 836 such references, or an average of more than 20 per page. The bibliographies imply a stupendous amount of reading, on account of which anyone who has ever attempted to reduce a wide field of literature to its essentials would pay sincerest homage to the author.

Possessing the fullest bibliography of any book on the subject in any language. *The Quaternary Era* has extraordinary potential value as a reference work. This is slightly marred by the format of the lists, which makes references difficult to track down, and by the fact that some important references of recent date are not included. However, a work of this magnitude and scope could hardly be expected to take account of a large number of very recent papers, in view of the currently rapid rate of growth of Pleistocene studies. In the largest sense one can say that Professor Charlesworth's comprehensive bibliographies present to students and libraries the considerable advantages of having nearly complete references within a single book. Although some readers may wish that the author had filtered the literature more completely so as to give greater emphasis to the leading or most useful titles, others will prefer the greater objectivity of a list that plays no favorites.

The arrangement of subjects, logical in most respects, in places is obscure. An example is the treatment of organisms. Chapter 34 (Pleistocene life) treats mammals first taxonomically and then stratigraphically. Chapter 35 (Early man) deals with human paleontology as well as with prehistoric cultures. All this precedes the main discussion of Pleistocene stratigraphy. Chapters 47 and 48, which follow that discussion, are essentially biogeographic, emphasizing habitat ranges and organic evolution. Partly because of this arrangement one has occasional difficulty, even with use of the excellent index, in running down

[1] American agent: St. Martin's Press, Inc., 103 Park Ave., New York 17.

specific topics concerning fauna and flora. Nevertheless, the topics when found add up to a very large amount of information.

In contrast, most of the material on meteorology and climatology is admirably brought together in a single chapter, where it is easy to find and, as usual, well fortified with references. The lengthy discussion of former land bridges, constituting part of Chapter 44, will be welcomed by biogeographers. The chapter entitled *Final dissolution*, treating the details of the last major glaciation, deserves the reader's especial attention, because it represents the field in which Professor Charlesworth has made his own principal contributions to knowledge. The four chapters on stratigraphy present their material in an admirably systematic manner and are accompanied by correlation tables.

The Quaternary Era contains much material in the form of abstracts, and other condensations of literature, not found in any other general summary of the Pleistocene. Much attention is devoted to discussion of genesis of Pleistocene features as well as to their description, and a great deal of thought has been given to the history of the development of concepts and of terminology. Of special interest to those concerned with this interesting history is the fact that many abandoned hypotheses of origin of geologic features are sketched by the author with appropriate references to the literature. A particularly informative example is Chapter 30, *Drift-ice versus land-ice*, an excellent summary and comparison of the glacial and diluvial theories of origin of drift.

Probably some readers would have preferred a fuller analytical treatment of current concepts; but those who are aware of the many occasions for controversy in this field will enjoy reading the extensive presentation of conflicting evidence and divergent opinions that the book offers. In places the author's tendency seems to be to try to reconcile opposed opinions with each other rather than to assess the probability of each, and in a sense this tendency is an expression of objectivity, for it gives the reader a large number of facts and a variety of hypotheses from among which he can choose. Certainly the book is an unrivaled source of information on the historical development of the concepts and nomenclature of glacial geology.

In three areas the discussion must be judged inadequate. Weathering and soils, the sedimentary record beneath the deep-sea floor, and isotopic methods of dating are making contributions of increasing importance to Pleistocene stratigraphy; yet here they are given little more than passing mention. Nor is there mention of the application of isotopic studies to former temperatures or to other problems apart from chronology. However, these inadequacies are more than offset by the rich and abundant discussion which Professor Charlesworth devotes to most other topics, some of which are discussed with much more informative detail than in any other general book. Notable among such topics is the variation of glaciers within the last few centuries and their relations to sunspot cycles and other phenomena. Another is the closely related treatment of postglacial climatic changes, with emphasis on the Climatic Optimum. Still another, of a different kind, includes ice shelves, icebergs, and sea ice. These examples give an idea of the remarkably wide range of material covered in this big book.

In summary, *The Quaternary Era* is primarily a book of reference, among the many strengths of which are its emphasis on the historical development of inference and concepts in glacial geology, and its colossal wealth of bibliography. As such it will surely be widely used for a long time to come, and will have enduring value. This reviewer could not afford to be without it, and it is safe to say that this opinion will be shared by a large number of those who work with Pleistocene problems. RICHARD FOSTER FLINT

Précis de Géomorphologie; by M. DERRUAU. P. 394; 164 figs. 50 pls. Paris, 1956 (Masson et Cie, 3,600 francs).—This general summary of geomorphology is intended as a textbook for undergraduates. Except for greater emphasis on geologic structure and less on the erosion cycle, the subject matter covered is approximately the same as that in most English-language texts. A brief introduction defines the scope of geomorphology and outlines the objectives and methods of the subject. The remainder of the book is divided into five sections: (1) constitution and movements of the crust, (2) the so-called normal erosion, (3) morphoclimatic systems of erosion, (4) influence of rock types on the nature and evolution of structural features, and (5) littoral and submarine morphology.

The flaws in this book considerably outweigh its merits. Most serious is the extremely generalized treatment of nearly all subjects. The various viewpoints on most controversial issues are not discussed; rather, in each such case a one-sided account is given and the matter dismissed with a dogmatic summary statement. To list only a few specific examples, the comments made about the nature of equilibrium slopes, importance of wind erosion in deserts, magnitude of rock disintegration by temperature changes, reasons for meandering of streams, processes active on pediments, and origin of submarine canyons seem to reflect ignorance of recent literature. As a result, the discussion gives no coherent picture of the development of geomorphic ideas, and the present status of knowledge in this subject. The short reference lists cited at the end of each section are by no means representative and even these are not referred to in the text. Most of the references listed are by French authors; very few pertinent papers in English are included, and to a slightly lesser extent the same is true for papers in other languages.

Many of the plates are well-chosen and carefully reproduced; however, some are out of focus and others, especially the several hachure maps, contribute little to the discussion. In quite a few cases the texts which accompany the plates and figures need further explanation or more definite geographic location. Another shortcoming is the lack of an adequate index; there is only an index to definitions of terms.

The recent revival of interest in geomorphology in North America has stemmed largely from application of quantitative techiques to measurements of land features and current erosional processes. Judging from Prof. Derruau's book, this approach has not yet affected geomorphic thought in France. Indeed, except for a few subjects, the treatment is so general that this book might just as well have been written a quarter of a century ago.

As a text, the presentation is much too cut-and-dried to be palatable to the inquisitive undergraduate. To the professional hopeful of obtaining an impression of what's new in French geomorphology, this book will be a disappointment. JOHN P. MILLER

The Barker Index of Crystals, a Method for the Identification of Crystalline Substances: Volume II, Parts 1, 2, and 3; by M. W. PORTER and R. C. SPILLER. P. v + 383; viii + ca. 730; viii + ca. 690, Cambridge, 1956 [issued 21 Jan., 1957] (W. Heffer & Sons, Ltd., £10).—The completion of the second volume, on the monoclinic system, marks an achievement of large proportions, for a substantial majority of the known, crystallizable substances are now covered. Volume I, published in 1951, covered 401 tetragonal, 434 hexagonal, and 2156 orthorhombic crystals; the current volume adds 3572 monoclinic crystals to the Index. The third and final volume probably will be the smallest. This work contains information on many compounds for which X-ray results are not yet available, and as such is an important addition to the library of every crystallographer; moreover, it presents useful determinative tables for all these substances. HORACE WINCHELL

Petrographic Modal Analysis; by FELIX CHAYES. P. 113; 15 figs., 12 tables. New York, 1956 (John Wiley & Sons, Inc., $5.50).—This authoritative text book is written primarily for advanced students and those professionally engaged in petrographic studies. It arose from a series of seminars on modal analysis given at the Californian Institute of Technology by the author and it fills a long felt need for a synthesis of this extremely useful analytical tool. The author is to be congratulated for presenting a unique and complete work, in which all the main contributions on this topic are reviewed. Hitherto unpublished material is also presented. The result is an up-to-date work based on sound statistical methods.

The whole technique, which most petrographers call the "Chayes method of modal analysis" is presented with the *quomodo et quando* of its use. The subject matter covers the geometrical and statistical basis of the method, a review of the techniques and instruments used, a critical study of reproducibility and the control of all possible errors.

The first chapter covers the geometrical basis of modal analysis. Modal analysis of banded rocks, the "bête noire" of this technique, has always caused many difficulties, but the second chapter offers good geometrical proof of its validity for almost any banded rock. In chapter 3, mention is made of the contributions of the initiators of this technique: Delesse, Rosiwal, and Shand. The various modern techniques of modal analysis are described.

The reproducibility of thin section analysis—i.e. the search for consistency in the results obtained for a series of operations carried out by one or many analysts—occupies chapters 4 and 5. The four following chapters cover the important question of analytical errors, which render this technique useless when errors exceed the required precision of measurement. Coarseness of grain, standards, limitations of the method, count length and the effect of

coarseness on the scale of petrographic investigation are discussed. Chapter 11, entitled "The Holmes Effect and the Lower Limit of Coarseness of Modal Analysis" is a warning to those who may be tempted to apply modal analysis to rocks unsuitable for application of this technique.

It is the reviewer's opinion that this elegant book will become a standard text and that it will be welcomed by all those engaged in the field of petrographic study. JEAN BERARD

Man, Culture, and Society; edited by HARRY L. SHAPIRO. P. vii, 380; 13 pls., 16 figs. New York, 1956 (Oxford University Press, $7.50).—The anthology "Man, Culture, and Society", composed of essays by seventeen scholars in the various fields of anthropology, is presented to the public as an introduction to the study of man. The editor invites the beginning student as well as the general reader with little or no background in this social science to acquaint himself with the basic ideas and materials of anthropology.

Since it is hardly possible to include in one volume all the important data and theories pertaining to "Man, Culture, and Society" the editor had to confine the content to fundamentals. The compact well-balanced opening chapter on evolution of man is followed by five articles dealing with methods and descriptive material from the field of archeology. The subsequent ten topical essays on language, culture, culture change, social structure, religion, and economics transfer the emphasis from content to principles governing the structure of culture and the processes of culture change. Although the reviewer realizes the difficulties connected with the selection of representative material and the necessity of eliminating data which may appear to be indispensable, he has, nonetheless, some critical remarks to make on the table of contents.

While evolution of man and linguistics are condensed into two well-organized and adequate chapters that offer the student the necessary background in these fields, the discussion of culture dynamics occupies fully three chapters. To give so much space to a single topic of one of the several branches of anthropology seems to be too generous, especially when one considers the fact that two fields of anthropology, namely culture and personality and applied anthropology have been completely omitted. The reviewer was also surprised by the lack of a general chapter on the nature of the science of anthropology and its relation to other systems of knowledge.

By employing several experts in the presentation of the data from such a diversified discipline, Professor Shapiro tries to preserve the authority of the original writings. Such authority is inevitably lost when a single author, who has to rely on secondary data in the fields which do not constitute his special interest, attempts to describe the whole subject by himself. The multi-author method employed in this volume has already been successfully used in specialized works addressed to advanced students in the discipline.

The carefully composed, well condensed and reasoned articles speak clearly for the advantages of the essay method. It is hardly conceivable that anyone else but authorities in the particular fields could write such brilliant chapters on languages, evolution of man, or culture change as those included in this volume. Although the presence of "the authority that comes from an

expert writing" has been clearly demonstrated in most of the essays, its achievement was not effected without some loss to the qualification of the volume as a textbook. Problems arise from the fact that the various authors have different approaches and assumptions which underly and modify their presentations of otherwise comparable material. While in a specialized compendium designated for advanced students who are familiar with much of the data the different approaches and assumptions serve as literary and philosophical seasoning, in an introductory text the same phenomenon may turn into a liability. It may give the beginning student a wrong impression from which he is liable to draw undesired conclusions. This is the case, for instance, where Prof. Movius' meticulously detailed, and well illustrated chapter on the Paleolithic is followed by Prof. Childe's analysis of the Neolithic, rich on generalizations and void of any figures. Although both chapters are most adequate and illuminating when considered as essays, their juxtaposition may produce in the beginner an impression of the paramount importance of differentiation among the Paleolithic cultures as contrasted with the Neolithic's "trivial differences". Similarly the concluding chapter on "How Human Society Operates" instead of summarizing the discussions on the various aspects of culture, redefines, due to its sociological approach, most of the concepts discussed previously.

This problem is, however, by no means unavoidable. Prof. Murdock (p. 250) shows a way in which to solve it. His clear statement about his orientation and the possible ways of approaching the study of culture change leaves no doubt on the part of the reader as how to interpret his essay.

"Man, Culture, and Society" constitutes more than a new text book on anthropology. It represents an important innovation which enriches not only the study of man but also Western Culture in general. It acutally exemplifies the process of innovation as "a new synthesis of old habits", those of a multi-author method applied to a text book, and thus it shows how an innovation "is dependent upon the existing content of the culture" (p. 251).

LEOPOLD POSPISIL

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